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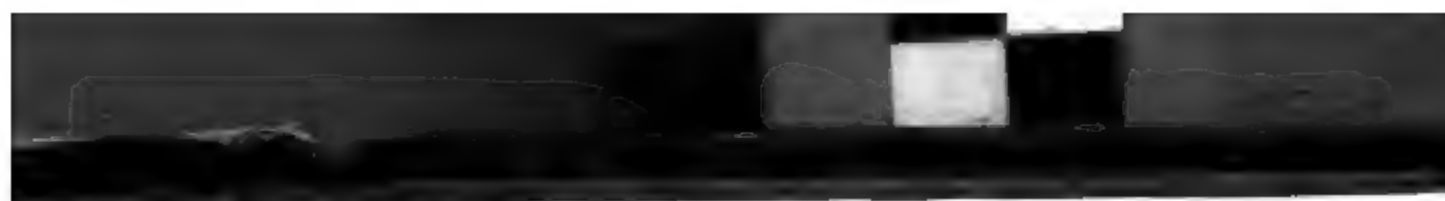
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LELAND STANFORD JUNIOR UNIVERSITY



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PROCEEDINGS

*Morton
Patt*

OF THE

ROYAL SOCIETY OF LONDON.

From June 16, 1870, to June 15, 1871.

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ERRATA.

Page 246, line 4, *for* 200 lbs. *read* 2 volts.

Page 287, line 27, *for* $v_0 e^{-\frac{250r}{l}}$ *read* $v = v_0 e^{-\frac{250r}{l}}$.

Page 288, line 29, *for* $Q = C \cdot \left[e^{-\frac{250R}{l}} - \frac{250R}{l} - 1 \right] R h^{\frac{1}{2}} l^{\frac{5}{2}}$,
read $Q = C \left[e^{-\frac{250R}{l}} - \frac{250R}{l} - 1 \right] R^{\frac{1}{2}} h^{\frac{1}{2}} l^{\frac{5}{2}}$.

Page 324, line 13 from bottom, *for* arterial *read* cardiac.

Page 372, line 7, *dele* comma *after the word* ether.

Page 374, lines 18 and 19, *for* "forms a sort of lather, on agitation from which"
read "forms on agitation a sort of lather, from which"

Page 375, line 22, *for* "alkaloids; and their derivatives thus"
read "alkaloids and their derivatives; thus"

" " 35, *for* "bromohydrobromate of tetracodeia."
read "hydrobromate of bromotetracodeia."

Page 498, line 2 from bottom, after "*action.*" insert:—"The mathematical expression of this law is $c = C p^{\frac{\log 3}{\log 2}}$, c being the chemical action, C the constant, and p the proportionate quantity of salt."

Page vii, line 14 from bottom, *for* Cuchullius *read* Cuchullins.

Correction to W. H. L. RUSSELL'S Paper on Linear Differential Equations.—No. IV.

The expression for Q , page 283, should be

$$Q = \dots \frac{A_n}{x^{n-\mu}} + \frac{A_{n-1}}{x^{n-\mu-1}} + \dots A_2 x^{\mu-2} + A_1 x^{\mu-1} + A_0 x^{\mu}.$$

The process for ascertaining the value of the integral

$$\int_a^\infty \frac{e^{xz}(z-\alpha)^{\lambda-1}}{(z-\beta)^{\mu+1}(z-\gamma)^{\nu+1}}$$

is erroneous, but how the mistake occurred I cannot now tell.—W. H. L. R.

DIRECTIONS TO THE BINDER.

- Plat'e I. between p. 6 & 7.
- II. & III. between p. 238 & 239.
- IV. to face 338.
- V. & VI. between p. 492 & 493.

PROCEEDINGS
OF
THE ROYAL SOCIETY.

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June 16, 1870.

XIV. "On the Atmospheric Lines of the Solar Spectrum, in a Letter to the President." By Lieut. J. H. HENNESSEY. Communicated by the President. Received May 21, 1870.

Mussoorie, April 25, 1870.

MY DEAR SIR,—I have the pleasure to enclose a map of the solar spectrum, including the region from the extreme red to the lines D, and a report on my endeavours to view the zodiacal light. A complete set of actinometric observations (simultaneous) between Dehra and Mussoorie has also been made. The latter observations are in course of reduction, and will, I trust, be submitted to you ere long.

2. Instruments.—The set of instruments which the President and Council of the Royal Society were good enough to send out to India under Lieut. Herschel's care, for my use, was duly handed to me at Bangalore (Madras Presidency) when I was on duty there in the winter of 1867–68. This set comprises a spectroscope with three flint-glass prisms, a hand-spectroscope, a tube with a double-image prism, and two of Hodgkinson's actinometers, as detailed in Professor Stokes's letter (list) addressed to the Under Secretary of State for India, dated 31 October, 1867.

3. Narrative.—On the completion of my duties at Bangalore I was enabled to bring the instruments up to Mussoorie, where the spectroscope was set up in May 1868. Meanwhile, however, the hot season in the plains had set in. The dust, as usual at this time of the year, filled the atmosphere, so that all hopes of obtaining spectroscopic observations of the sun about the time of sunset were in vain ; and my endeavours to carry out the suggestions of the Committee, which they were so good as to make in connexion with my letter of 13 Feb. 1866, were reluctantly deferred to the ensuing October, when a clear atmosphere might again be expected.

4. *Position of observatory.*—With the permission of Colonel Walker, R.E., Superintendent of the Great Trigonometrical Survey of India, the spectroscope was placed in the small rotating dome observatory of the Survey department. It stands on the southernmost range of the Himalaya Mountains, in lat. N. $30^{\circ} 27' 41''$, long. E. $78^{\circ} 6' 45''$, height above mean sea-level 6937 feet. My observations were made in this observatory, excepting the interval last winter, when, in order to command a view of the sun down to the horizon, I shifted the instrument to an adjoining ridge.

5. *Times of observation.*—I was not long in ascertaining that between 10 A.M. and 2 P.M. no sensible alteration in the spectrum occurred. I am here speaking of the red end, which has almost exclusively been the subject of my study. Accordingly, the observations which may be called "sun high" were made between these hours. It was, however, some time before I discovered that by far the greater effect of the earth's atmosphere on the spectrum did not occur until the sun was about to dip* under the horizon. In fact it is only when the sun is some three or four diameters from the horizon that the very considerable alteration of the spectrum begins. To secure this condition, the atmosphere must be quite free from dust, which, as already implied, rises in great clouds from the plains until deposited by the heavy rains in July and August; and the sky must be clear of clouds, at least about the horizon. The lines were watched under these circumstances, and mapped down to the last moment before any serious diminution of light occurred. These results are given in the map observed "at sunset." It is easy to see that such favourable conditions cannot be expected continuously day after day; and even when available, the time for observation is so very limited, that no results can be obtained without considerable perseverance. When, however, this exceptional condition of air and sky do occur, the observer who has watched and become familiar with the spectrum sun high, is well rewarded by the decided manner in which the atmospheric lines now stand out, as the sun, still quite bright, is on the point of setting.

6. *Narrative resumed.*—Between May and October 1868 I employed my leisure in watching the spectrum sun high, fully expecting that the brilliant weather of the autumn would enable me to make every endeavour towards carrying out the suggestions of the Committee with the sun low. But the autumn came and the air was still hazy. In fact, while the average fall of rain here is some 90 inches, there fell in the season of 1868 only 61 inches, or about two-thirds the usual quantity; a drought which in the first instance led to the scarcity of food, from which these provinces are only now emerging. To complete my mortification, I found that the colli-

* A range of hills to the eastward conceals the view for some degrees in altitude; so that, though I have repeatedly watched the spectrum of a morning as well, my map is in preference based on the view at sunset, when the sun can be followed down to horizon.

mator of the spectroscope had bent, by its own weight, at the flanges where it unites with the drum; and this deterioration involved remedies for which the available means were but too limited. In this way the autumn of 1868 passed away, to my great disappointment, without bringing me any results save experience; and I eventually left Mussoorie, as usual, for the winter of 1868-69, to carry on my survey duties at Dehra.

7. *Narrative continued.*—On my return to this place about the end of April 1869, I found, as was to be expected, that, owing to the small amount of rain in the winter, the hazy state of the atmosphere still continued; so that it was not until last October (1869) that I was able fairly to commence work at sunset. Even then I had to discover by experiment that, to observe the full atmospheric effect on the spectrum, I must command a view of the sun down to the very horizon. To suit this new condition, I had to shift the spectroscope twice to adjoining positions, and to modify the map I had already prepared.

8. *Construction of spectroscope.*—The spectroscope is made up of a drum containing three flint-glass prisms, one of which may be made non-effective at pleasure. The collimator unfortunately consists of three pieces, of which the nearest portion is rigidly fixed to the drum. The second piece unites with the drum piece by means of three screws driven through flanges; the third piece, which unscrews for packing, bears the slit at its far end. The spectrum is viewed by a telescope provided with four eye-pieces, whose magnifying-powers I determined with a dynameter to be as follows:—

Eye-piece.	Power.
No. 1	9
No. 2	17
No. 3	25
No. 4	54

A graduated circle and vernier, read by a side-telescope, provide the means for measuring distances between the lines; and the foregoing are mounted equatorially on a tripod-stand.

9. *Details.*—In observing the spectrum sun high, I was soon struck by the difference in intensity of the lines as seen at Mussoorie and as given in Kirchhoff's well-known map. Excepting the decided lines A, B, C, and D, all the others appeared comparatively faint, and even wanting in definition. It is also worthy of remark, that while Kirchhoff used a power of forty to view the spectrum with, I am unable to employ any higher power than eye-piece No. 2 (power 17). My observations were thus made with eye-piece No. 2, using all three prisms; and this combination, the most powerful at my command, produced an image only some three-tenths of what Kirchhoff, I presume, actually saw.

10. *Details.*—The comparative smallness of image presented to view has necessarily added sensibly to the difficulties of the undertaking; for,

understanding the Committee to suggest that I should adopt Kirchhoff's map *and scale* as a basis, it became incumbent on me to magnify the image actually viewed some three times before sketching it on my map. As an aid towards this end, I removed the strong metal *cross* wires in the telescope, and replaced them by two tolerably stout silk fibres, *parallel* to one another, and rather more apart than the image of B sun high. I could thus compare a given line with one of the fibres, or, where more convenient, with the space between them; and, through the intervention of the fibres, compare any one line with any other. These comparisons of magnitude were thus all mental, there being no micrometer in the eyepiece to measure small spaces with, and the smallest quantity that I could measure on the circle being the distance between the two lines in D. In selecting a relative unit between Kirchhoff's and my map, I have adopted the breadth he has assigned to the line A. This line will therefore be found equally wide in his map and in mine at sunset. The necessity for enlarging the image leads to an unintentional deception, deserving notice. It will be seen that my line A, for instance, is *equally* and intensely black throughout; whereas the same line, shown at the same width in Kirchhoff's map (402 to 407), is made up of several lines of varying intensity. This difference is due to the lesser power of my instrument; and in all probability the line up here would decompose into its component lines under greater dispersion.

11. *Necessity for constructing my map from independent measures.*—The want of intensity generally in the spectrum sun high at Mussoorie, combined with the smaller power of the instrument, made it exceedingly difficult, and in most cases impossible, to identify individual lines in Kirchhoff's map with corresponding ones under view; so that, after making every endeavour at identification, I was obliged to content myself with adopting the positions (sun high) assigned by him to the strong lines A, B, C, and D, and to place all the other lines of sensible intensity by means of differential measures and interpolation. Practically speaking, this amounted to the construction of a new map, so far as my wants were concerned.

12. *Definitions.*—By a constant line is intended one that presents the same appearance at sunset or sun high; a variable grows darker at sunset, and, generally speaking, widens out; and an atmospheric or air-line is invisible only sun high. By a band may be understood those broad belts which suddenly appear like shadows at sunset; instances of bands occur on both sides of C and elsewhere.

13. *Lines mapped.*—*Every* variable or air-line (or band) sensibly visible has been measured and mapped; but of the constant lines only those sufficiently intense to be easily intersected were observed and placed. It appeared undesirable to crowd the map with a large number of faint lines, whose property of constancy made their presence in an atmospheric map redundant. On the other hand, the introduction of certain constant and

sufficiently intense lines proves valuable for purposes of identification. I may add in this place, that no lines whatever are here visible between B and C; and this fact will afford some means of estimating the relative intensity of spectrum viewed at Mussoorie and that mapped by Kirchhoff.

14. *Direct sunlight employed.*—On first commencing work, I endeavoured to follow the plan adopted by Kirchhoff and employ a heliostat for reflecting the sun's rays; but, unlike the Professor, I was unable to command the clockwork for driving the heliostat. The variability of light under these circumstances proved intolerable; add to this the necessity for observing the sun as late in the evening as possible, made the introduction of any absorbing medium undesirable; and, lastly, the object of maintaining a constancy of circumstances between sun high and sunset, led me to prefer pointing the collimator direct to the sun. I therefore screened the drum and its prisms with an ample sunshade, and received the light from the sun directly on to the slit of the spectroscope.

15. *Identification of constant lines.*—It will be seen that in the space A to C there appears but little for recognition. No doubt the following groups are, generally speaking, identical, viz.:—

My map, sun high.	Kirchhoff's map.
Group 469–492	is the same as 470–492.
„ 499–508	„ „ 498–509.
„ 570–586	„ „ 570–587.

But the component lines, group by group, are widely different, and several sensibly intense lines in the Professor's map between 541 and 564 are absent here.

16. *Constant lines, continued.*—In the space C to D the identification is more frequent and definite. My 711 and 719 are clearly represented in Kirchhoff's map, and between the latter line and 795 several other cases of identity occur. Further on, towards D, the resemblance is not clear; until on arrival at the variable lines between my 955 and D recognition becomes impossible. Remembering that my map has been prepared from perfectly independent measures, I am naturally glad of establishing identification, where possible, with the Professor's map, for the evidence such identity offers of the accuracy of my work. Until my map had been *finished*, and the scale of millimetres drawn above it, the means of making a comparison were all wanting.

17. *Comparison of variable and air-lines.*—In discussing the variable and air-lines, I much regret my inability to adopt the suggestions of the Committee, and institute a comparison with Sir David Brewster's and Dr. Gladstone's map given in the Philosophical Transactions for 1860. The volume in question is, unfortunately, one of the few of its kind not at present included in the library of the Great Trigonometrical Survey; nor am I aware that any other library in these provinces possesses a copy. The only map of the kind to which I have access is that by Janssen, given in

Roscoe's Spectrum Analysis, fig. 57. The scale adopted in this diagram is, however, only about half that of Kirchhoff's, and the lines are so very numerous that identification is exceedingly difficult. It is evident, however, that my 812, a notably variable line, is identical with his 254.

18. *The same, continued.*—Comparing with the lines numerically alluded to by Kirchhoff in his appendix as atmospheric lines, the following coincidences (nearly) appear :—

My map at sunset.	Kirchhoff's list of atmospheric lines.
711.....	711·4
955.....	954·2
961.....	961·
967.....	{ 965·7 } { 968·7 }
971.....	970·5
973.....	972·1
977.....	977·4
987.....	988·9
990.....	989·6

In assuming the foregoing identities, I have been guided necessarily merely by numerical coincidences. *All* the foregoing, in the proposed nomenclature, are variable lines, not air-lines.

19. *Discussion of air-lines and bands.*—Again, comparing my map sun high with that at sunset, and beginning from the red end, the first group met with are the air-bands 262 to 302. These bands, so far as I am aware, have not been placed before, if seen. I have seen and taken measures to them repeatedly; but without the interposition of a blue glass they were invisible. The slit, too, must be sensibly widened. The whole group is nearly at the end of the luminous rays, where, at sun high, almost complete darkness prevails; and after careful watching of this group, I cannot help entertaining the belief that the *range* of the luminous rays *increases* towards the red end of the spectrum *as the sun approaches the horizon*. With the sun high, hardly any light reaches so far as 262 of the scale. When, however, the sun is setting, this part of the spectrum lights up sensibly. I am even inclined to assert that other bands exist still further removed from A than the group under notice; they, however, appeared too faint for intersection. May it not be anticipated that, at considerable terrestrial altitudes, an extension of the spectrum will occur both at the violet and red ends?

20. *The same, continued.*—Between 508 and 570 a very marked succession of air-lines and bands occur. Indeed, so far as I have been able to glance at the spectrum *in general*, the most refrangible rays appear the most liable to absorption. The air-bands about C and D are worthy of notice, as also are the air-lines 724 and 726.

21. *Discussion of variable lines.*—Both A and B appear as lines of this

class. In the absence of a micrometer for measuring small spaces, I have assumed that in widening out at sunset A does so equally on both sides; on the contrary, B, I believe, maintains its left (or violet) edge constant, and changes towards its right (or red) edge. The line 812 is the most notably variable I have observed. Unlike other lines, it commences to change quite two to three hours before sunset. The darkening is gradual until near sunset, when it suddenly becomes black, and the band to the right as suddenly appears. The variable lines are, however, so numerous, that further notice of them might prove tedious reading. I therefore content myself by calling attention to the fact that, so far as I have the means of judging, the variable lines at sunset and sun high superpose one another,—more accurately speaking, on the former occasion, when they are generally wider, they include the position occupied sun high. I need hardly dwell on the circumstance that the loss of light which no doubt occurs near sunset *cannot* be the cause of the widening out in these lines; for constant lines, identified with Kirchhoff's, stand mapped in the vicinity of variable lines of greater and lesser refrangibility.

22. *Concluding remarks.*—At the time of writing the air is so laden with dust that you may gaze on the setting sun with impunity; and spectroscopic observation, even at several degrees of altitude, is impossible. But when the ensuing rainy season is approaching its termination, and the exquisitely clear state of atmosphere once more returns, I hope to make good progress with the map; and also to try and ascertain*, by comparison of the map already made with the actual objects, if the variable and atmospheric lines and bands change their appearance from time to time. A variation of the latter kind might open out a vast field of inquiry.

23. *Proposed alteration of spectroscope.*—Though quite willing meanwhile to continue the work with the spectroscope as it now stands, I should consider it a great boon if you would allow me to send it to England for certain improvements. These I briefly indicate as follows:—1. One or even two additional prisms might be introduced into the drum, so as to increase the dispersion. 2. The collimator should be made up into *one* piece with the drum, and this may be done simply by uniting the present three pieces firmly together. 3. A micrometer introduced at the eye-end of the telescope would be invaluable. The stand answers every purpose, so that it might be useless sending it home, unless any delay is likely to occur in improvising a stand to adjust and test the instrument. Should I receive a favourable reply by the return mail, the spectroscope ought to reach Messrs. Troughton and Simms by the end of July; and if they could undertake to do the needful alterations by the end of August, it would, if despatched immediately overland, reach me about the first week in October, just in time for the fine weather. Unless, however, these dates are not exceeded, I should lose the observing season, and this would be lamentable.

24. *The same.*—Even should the foregoing meet with approval, I am

* Report of the Committee, last paragraph of § 4.

aware that without suitable supervision the alterations could not be carried out satisfactorily ; and in saying this, I make no doubt that Messrs. Troughton and Simms would perform their part in their usual effective manner. I already owe Mr. Huggins a considerable debt of gratitude for past superintendence ; and his kindness on that occasion induces me to venture the hope that he would undertake to direct such alterations as may be approved of with despatch.

25. *Zodiacal light.*—Every attention has been given to this subject, but the only result obtained is, that no traces whatever of the zodiacal light are visible here. I was on the watch in October and the early part of November 1868. In April and May of 1869 my absence on duty at Dehra prevented my devoting more than an occasional evening to the subject. Last autumn, however, I gave it every attention, but all without seeing the light. The following extract from my note-book, made the first morning of my watching last winter, is a representative of all subsequent entries :—

13th October, 1869. Mussoorie Observatory.—A beautifully clear morning. Not a speck of cloud or mist to be seen. The horizon, S.S.E. by S. to S.S.W., appears sharp and distinct, though some 90 miles distant. To the north, the snowy range, though some 60 miles away, appears hardly a day's journey from this. The light is gradually increasing ; the red rays now tinge the sky ; the sky itself brightens ; the highest peaks of the snowy range are tipped with light ; the sun bursts over the hills as if in a moment. All this has happened without any wings or rays preceding the sun. In other words, no traces of the zodiacal light have appeared.

26. *The same, continued.*—I watched for the light on the mornings of the 14th, 18th, 21st, 23rd, and 29th October, and on the 1st, 9th, and 15th November, 1869. On all these occasions the air and sky were beautifully clear. Other dates of watching, under unfavourable circumstances, are excluded from this list. Though no zodiacal light appeared to me at Mussoorie, it is worthy of remark that Colonel Walker, while travelling by post across the plains of Central India, saw a brilliant show of the light (as far as I can reckon) between the 12th and 15th of November, 1869. Recently I watched for the light on the evenings of the 11th, 12th, 13th, and 14th April, 1870, with the same results as before.

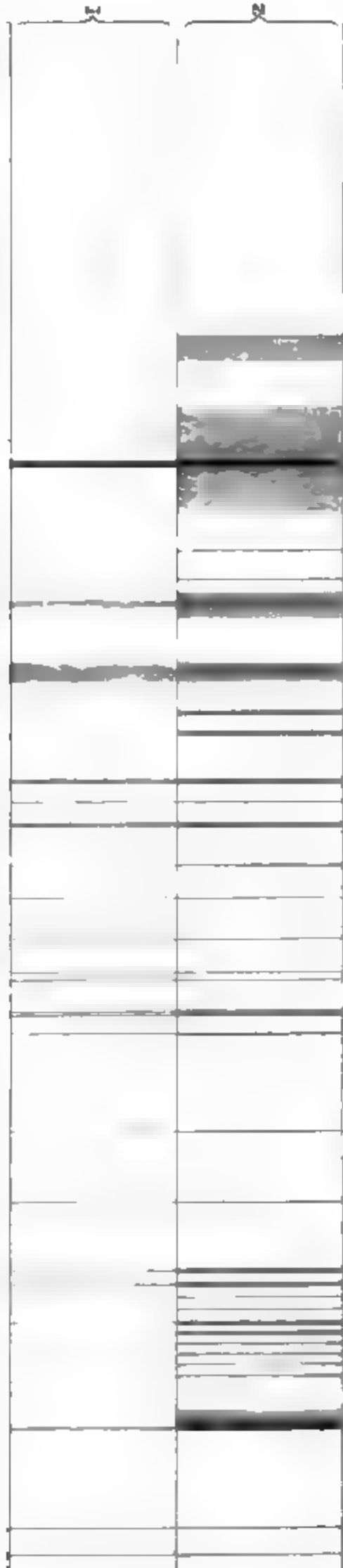
27. *The same, conclusion.*—How far these facts supply a connecting-link between the earth's atmosphere as a medium and the zodiacal light I leave for the consideration of more competent authorities. The information may have some special interest for Mr. Balfour Stewart, who, I learn from the monthly notices of the Royal Astronomical Society, has given the subject some attention.

28. *Actinometrical observations.*—Discussion of these observations is deferred until, with the assistance of Mr. W. H. Cole, M.A., I am able to reduce and forward the results already obtained. The two actinometers (Hodgkinson's) work capitally, and are in good order. Amongst other observations, they have been compared with one another. Unfortunately

D 101 100 99 98 97 96 95 94 93 92 91 90 89 88 87 86 85 84 83



C 82 81 80 79 78 77 76 75 74 73 72 71 70 69 68 67 66 65 64



B 63 64 65 66 67 68 69 70 71 72 73 74 75 76 77 78 79 80 81 82 83 84 85 86 87 88 89 90 91 92 93 94



A 45 46 47 48 49 50 51 52 53 54 55 56 57 58 59 60 61 62 63 64 65 66 67 68 69 70 71 72 73 74 75 76 77 78 79 80 81 82 83 84 85 86 87 88 89 90 91 92 93 94



W. W. L. L.

1 PART OF THE SOLAR SPECTRUM AS SEEN BETWEEN 10 A M & 2 P M
2 PART OF THE SOLAR SPECTRUM AS SEEN AT SUNSET.



a press of other work precluded them from comparison with the instrument (standard) at Kew Observatory. After inspection of the observations, you may be good enough to consider the desirability of sending me a third actinometer of the same kind, due comparison being first made at Kew, whereby the relation between the instrument at the Observatory (Kew) and those sent out to me by the Royal Society may be established.

XV. "On the Radiation of Heat from the Moon.—No. II." By
the EARL OF ROSSE, F.R.S. Received June 14, 1870.

In a former communication to the Royal Society I gave a short account of some experiments on the radiation of heat from the moon, made with the three-foot reflector at Parsonstown, during the season of 1868–1869. I then showed :

1st. That the moon's heat can be detected with certainty at any time between the first and last quarter, and that, as far as could be ascertained from so imperfect a series of observations, the increase and decrease of her heat, with her phases, seems to be proportional to the increase and decrease of her light, as deduced by calculation*.

2ndly. That a much smaller percentage of lunar than of solar rays is transmitted by a plate of glass, and we therefore infer that a large portion of the rays of high refrangibility, which reach the moon from the sun, do not at once leave the moon's surface, but are first absorbed, raise the temperature of the surface, and afterwards leave it as heat-rays of low refrangibility.

3rdly. That, neglecting the effect of want of transparency in our atmosphere, and assuming, in the absence of any definite information on the subject, that the radiating-power of the moon's surface is equal to that of a blackened tin vessel filled with water, the lunar surface passes through a range of 500° F. of temperature ; consequently the actual range is probably considerably more.

4thly. The proportion between the intensity of sunlight and moonlight, and between the heat which comes from the sun and from the moon, as deduced from those observations, agreed as nearly as could be expected with the values found by independent methods, and for this reason might be considered the more reliable.

During the past season these observations have been continued, but much time has been spent in trying various modifications of the apparatus, and a satisfactory comparison of observations made on different nights, under different circumstances, has been impossible ; however, by more numerous and more complete experiments, made alternately with and without an inter-

* See the Proceedings of the Royal Society, No. 112, 1869, page 439.

posed plate of glass, the second conclusion arrived at during the previous season has been to a great extent confirmed.

The following Table gives the values found for the percentage of the moon's heat which passes through glass :—

Date of observation.	Distance of moon from opposition.	Altitude of the moon.	Percentage of moon's heat transmitted by glass.	
April 15th, 1870	0	0		I.
	5	{ 15	13.3	II.
		{ 20½	15.5	III.
		{ 24	16.6	I.
April 16th	15	{ 15	14.5	II.
		{ 24	14.6	I.
April 17th	31	{ 19½	10.0	II.
		{ 20	10.0	
March 13th	50	50	7.1	
February 10th ...	66	44	8.4	
February 9th	77	32	9.3	
April 9th	81	{ 44	11.0	I.
		{ 16	11.0	II.
April 8th	93	30	12.0	
March 8th	109	27	13.0	

Mean = 11.88.

The same plate of glass which was used in I. and II. on April 15th, and the experiments on the two following nights, was tested for the solar rays, and the following values of the percentage of heat transmitted were obtained :—

April 15th.....	86.2
	86.6
April 18th.....	89.3
	84.3
	87.1
Mean on April 18th.....	86.8

The piece of glass used on the other occasions, instead of being placed at six or eight inches from the pile, was laid against the end of the protecting cone, or about half an inch from the face of the pile. When it was placed in this position and tested for solar rays, an increase of deviation in the proportion of 1.1 to 1 was obtained, owing to the "bottling up" of the sun's rays as in an ordinary greenhouse, and the keeping off of currents of air.

It seems therefore to be clearly proved that there is a remarkable difference between the sun's and the moon's heat in regard to their power of passing through glass. The amount transmitted varies from night to night, and in the later observations the value was generally larger than in the earlier ones. Possibly this may have arisen from the formation of a

slight and imperceptible film of moisture on the surface of the glass, which was much more unlikely to form during the much shorter period* of exposure to the night air in the later observations.

The experiment made during the previous season to determine the ratio between the heating-power of the moon and of the sun was repeated with more care, and the value found, taking what appeared to be the most probable mean heating-power of full moon, as determined on various nights, was

$$\frac{\text{Sun's total heat}}{\text{Moon's total heat}} = 82600.$$

Taking† the percentage of light transmitted by glass = 92

Do. do. of sun's heat = 87

Do. do. of moon's heat = 12

Do. do. of heat from a body at 180° F. = 1.6

If $\frac{0}{0+l}$ and $\frac{l}{0+l}$ represent respectively the percentage of dark and luminous rays present in the moon's radiant heat, and $\frac{0'}{0'+l'}$ and $\frac{l'}{0'+l'}$ the corresponding quantities for the sun's radiant heat, we have

$$\frac{0 \times .016 + l \times .92}{l + 0} = .12,$$

and

$$\frac{0' \times .016 + l' \times .92}{l' + 0'} = .87;$$

$$\therefore \frac{l'}{l} = \frac{0+l}{0'+l'} \times \frac{854}{104} = 82600 \times \frac{854}{104} = 678300.$$

In all the foregoing experiments on lunar radiation the quantity measured by the thermopile was the difference between the radiation from the circle of sky containing the moon's disk and that from a circle of sky of equal diameter not containing the moon's disk; we have obtained no information in reference to the absolute temperature of either the moon or the sky.

The following experiment was therefore made with the view of trying to connect the radiation of the sky with that of a body of known temperature, the deviation due to each degree (Fahrenheit) difference of temperature between a blackened tin vessel containing hot water and subtending a given angle at the pile and a similar vessel containing colder water was first ascertained; then a similar determination of that due to the difference of radiation from one of these vessels, and from a portion of sky of equal diameter, was made. The following was the result:—

* About 12 minutes in place of 30 to 60 minutes.

† All these values, except the first, were determined by experiment for the specimen of glass employed.

	Altitude of part of sky examined.	Calculated difference of tem- perature.	Tempera- ture of tin vessel.	Apparent tempera- ture of the sky.	Remarks.
April 16th	49	23·9	55·5	31·6	Sky hazy. } Sky apparently black and transparent; occasional light clouds.
April 20th	49	32·7	49	16·3	
"	23½	28·3	51	22·7	
"	50	28·3	50·5	22·2	
"	64	30·1	47	16·9	
"	64	26·2	44	17·8	

If the temperature of space be really as low as is supposed, this result seems to indicate considerable opacity of our atmosphere for heat-rays of low refrangibility.

The ever varying transparency of our atmosphere has been found to be a very serious obstacle; but the much greater steadiness of the needle during the later experiments (the mean error of the last few nights' observations having been from two to three and a half per cent. only of the whole deviation*) encourages us with the hope that, by taking advantage of favourable moments, and measuring the moon's light simultaneously with her heat, more accurate information on this subject may soon be acquired.

The observations were examined with the view of ascertaining how far the heating-power of the moon's rays varies with her altitude. Owing to the interference of clouds, and the limited range of altitude within which the observations were made, it is hardly worth while to give the results in detail; however, I may just say that the heating-power of the moon's rays appears to diminish with her altitude only about one-third as fast as the intensity of the solar chemical rays, as ascertained by Roscoe and Thorpe.

An attempt was made to ascertain, by comparing two measurements of the moon's light at different altitudes with two corresponding measurements of her heat, whether our atmosphere intercepts the *heat*-rays to a greater extent than the *luminous* rays. It was found that while the light was diminished with the altitude in the proportion of about 3 to 1, the heat was diminished in the proportion of about 5 to 1. In consequence, however, of much of the moon's light and heat being intercepted by hazy clouds, or *condensed* vapour, at the lower altitude, the experiment was inconclusive as to the effect of a *transparent* atmosphere on the dark rays of heat.

The accompanying diagram shows the proportion between the amount of lunar heat found on various nights at various ages of the moon. There appears to be a general accordence between the variation of her radiant heat with her phase and the corresponding amount of her light as deduced by calculation.

* During the experiments of the previous season the mean error varied between 27 per cent. and 85 per cent. or more.

14 Mr. W. H. L. Russell on *Linear Differential Equations*. [June 16,

Subjoined is a Table giving the dates of the various observations, with the reference numbers corresponding to those on the diagram, and with remarks on the state of the sky.

Number in diagram.	Date of observation.	Remarks.
I.	April 4th	No mention of cloud.
II.	January 8th	
III.	April 8th	
IV.	January 9th	No mention of cloud.
V.	March 8th	Extremely clear sky.
VI.	April 9th	No mention of cloud. [night by a halo.
VII.	January 10th ...	Sky not good ; thin hazy clouds, followed later in the
VIII.	February 9th.....	
IX.	January 11th.....	Much wind.
X.	February 10th ...	No mention of clouds.
XI.	January 12th.....	Occasional small clouds, and rather hazy.
XII.	November 19th ...	Clouds producing prismatic colours round the moon.
XIII.	March 13th	Sky not good ; fleecy clouds. [clouds.
XIV.	April 13th	
XV.	April 14th	Bad night ; stopped after 10 minutes, in consequence of
XVI.	April 15th	Sky very clear.
XVII.	January 16th.....	Occasional clouds.
XVIII.	September 20th ...	
XIX.	February 16th ...	Sky hazy at sunset ; occasional clouds. [night.
XX.	April 16th	Sky apparently not quite so clear as on the preceding
XXI.	April 17th	
XXII.	November 22nd...	Fog and white frost, afterwards drift.
XXIII.	November 23rd...	No remark about cloud.

XVI. "On Linear Differential Equations."—No. III. By W. H. L. RUSSELL, F.R.S. Received June 11, 1870.

The integrals obtained in my last paper on this subject were deduced by the same process which afforded the determinants in the first paper. It is obvious that these integrals could be found by a more direct investigation. This is what I am now going to attempt. It will be found moreover that the present method will have the advantage of clearing away the ambiguities arising from the existence of common factors in the algebraical coefficient of the highest differential, and the denominator of the exponential in the solution. It will also be found to lead us to certain ulterior results.

Let us take the differential equation

$$(a + \beta x) \frac{d^3 y}{dx^3} + (\alpha' + \beta' x + \gamma' x^2) \frac{d^2 y}{dx^2} + (\alpha'' + \beta'' x + \gamma'' x^2) \frac{dy}{dx} + (\alpha''' + \beta''' x + \gamma''' x^2) y = 0.$$

Let us now put in this equation

$$y = E(x) e^{\int u x (\mu + \nu x)}.$$

We shall easily see that it is impossible for the exponential to contain

higher powers of (x) than here given. Then we shall have

$$\begin{aligned}\frac{dy}{dx} &= \left\{ \frac{dE}{dx} + (\mu + vx)E \right\} e^{\int dx(\mu + vx)}, \\ \frac{d^2y}{dx^2} &= \left\{ \frac{d^2E}{dx^2} + 2(\mu + vx) \frac{dE}{dx} + (\mu + vx)^2 E + vE \right\} e^{\int dx(\mu + vx)}, \\ \frac{d^3y}{dx^3} &= \left\{ \frac{d^3E}{dx^3} + 3(\mu + vx) \frac{d^2E}{dx^2} + 3(\mu + vx)^2 \frac{dE}{dx} + (\mu + vx)^3 E \right. \\ &\quad \left. + 3v \frac{dE}{dx} + 3v(\mu + vx)E \right\} e^{\int dx(\mu + vx)}.\end{aligned}$$

Substituting these values in the differential equation, and equating the coefficients of the highest powers of (x) to zero, we have

$$v^3\beta + v^3\gamma' = 0,$$

whence

$$v = -\frac{\gamma'}{\beta},$$

and also

$$v^3\alpha + 3v^3\mu\beta + 2\mu v\gamma' + v^3\beta' + \gamma''v = 0;$$

whence substituting for (v) and reducing, we have

$$\mu = \frac{\alpha\gamma'}{\beta} + \frac{\gamma''}{\gamma'} - \frac{\beta'}{\beta},$$

as before.

The other integrals given in my last paper may be deduced in a similar way.

This method suggested to me that it was possible to ascertain if any linear differential equation admitted of a solution of the form $y = \frac{P}{Q} e^{\frac{p}{q}}$, where P, Q, p, q are rational and entire functions of (x) .

Let, as before, the differential equation be

$$(\alpha_0 + \alpha_1 x + \dots + \alpha_m x^m) \frac{d^m y}{dx^m} + (\beta_0 + \beta_1 x + \dots + \beta_n x^n) \frac{d^{n-1} y}{dx^{n-1}} + \dots = 0.$$

Then it is easily seen that the factors of q must be divisors of

$$\alpha_0 + \alpha_1 x + \dots + \alpha_m x^m;$$

hence if we have

$$\alpha_0 + \alpha_1 x + \dots + \alpha_m x^m = (x-a)^{r'}(x-b)^{r''} \dots,$$

we must have

$$y = \frac{P}{Q} e^{\eta_0 + \eta_1 x + \eta_2 x^2 + \dots + \eta_\mu x^\mu} + \frac{A_r}{(x-a)^r} + \frac{A_{r-1}}{(x-a)^{r-1}} + \dots + \frac{B_s}{(x-b)^s} + \dots$$

Now this series can evidently be written in the form

$$y = R(x) e^{\eta_0 + \eta_1 x + \dots + \eta_\mu x^\mu},$$

where $R(x)$ can be expanded in descending powers of (x) . Hence if this value of y be substituted in the proposed differential equation, we may determine $\eta_0, \eta_1, \dots, \eta_\mu$ by the same process as before.

To determine A_r, A^{r-1}, \dots , let

$$x = \frac{1}{x-a}, \text{ or } x = a + \frac{1}{x}.$$

Then the solution of the resulting linear differential equation in (x) will be of the form

$$y = R_1(x) e^{A_0 + A_1 x + A_2 x^2 + \dots + A_r x^r};$$

where $R_1(x)$ can be expanded in descending powers of (x) , and therefore A_0, A_1, A_2, \dots be determined as before.

In the same way, by putting $x = b + \frac{1}{x}$, we may determine B_0, B_1, \dots

In order to exhibit the accuracy of this reasoning, I will form some differential equations from given primitives, and then see if the above process will enable us to reproduce these primitives as solutions.

Let us take the primitive

$$y = (x+1) e^{x^2 + 3x + \frac{1}{x-1}}.$$

From this we may deduce the differential equation

$$(x^3 - x^2 - x + 1) \frac{d^2 y}{dx^2} - (x^4 + 2x^3 - 6x^2 - 3x + 4) \frac{dy}{dx} - (2x^5 + x^4 + 4x^3 - x^2 - 5x - 4)y = 0.$$

Let

$$y = R(x) e^{\int dx(\mu + \nu x)}.$$

If we use higher powers of (x) in the exponential, they will not give us a result.

Substituting in the differential equation the values of $\frac{dy}{dx}, \frac{d^2 y}{dx^2}$ given in the earlier part of this paper, and equating the highest terms to zero, we have

$$\nu^2 - \nu - 2 = 0 \text{ and } 2\mu\nu - \nu^3 - \mu - 2\nu - 1 = 0,$$

whence

$$\nu = 2 \text{ and } \mu = 3, \text{ or } \nu = -1, \text{ and } \mu = 0,$$

and therefore

$$\int dx(\mu + \nu x) = x^2 + 3x, \text{ or } -\frac{x^2}{2}.$$

The divisors of the first term are $x-1$ and $x+1$.

Let $x = \frac{1}{z} + 1$, and the differential equation becomes

$$(2z^7 + \dots) \frac{d^2 y}{dz^2} - (2z^7 + \dots) \frac{dy}{dz} + (3z^5 + \dots)y = 0,$$

which gives a solution of the form $y = R_1(z) e^z$, when $z = \frac{1}{x-1}$. If we put

$x = \frac{1}{z} - 1$, the differential equation will be of the form

$$(Az^8 + \dots) \frac{d^2 y}{dz^2} + (Bz^7 + \dots) \frac{dy}{dz} + (Cz^5 + \dots)y = 0,$$

in which the numerical coefficients are of no consequence, as the equation does manifestly not admit of an exponential solution. If, then, the differential equation admits of a solution in the proposed form, it must be one of the two forms,

$$y = R(x)e^{x^2+2x+\frac{1}{x-1}} \text{ or } y = R(x)e^{-\frac{x^2}{2}+\frac{1}{x-1}},$$

where $R(x)$ is a rational and entire function of (x) or a rational fraction. Using the first form, we should of course determine it equal to $x+1$.

As a second example, we form from the primitive, $y = (x-1)e^{x+\frac{1}{x+1}}$, the equation

$$(x-1)(x+1)^2 \frac{d^2y}{dx^2} + (2x^2+x-3) \frac{dy}{dx} - (x^3+5x^2+4x+1)y=0.$$

Here we must put $y = R(x)e^{\int \mu dx}$, higher powers of (x) in the exponential leading to no result. Substituting, we find $\mu = \pm 1$. Let $x = \frac{1}{z} - 1$, and the differential equation becomes

$$(2z^3 + \dots) \frac{d^2y}{dz^2} + (2z^3 + \dots) \frac{dy}{dz} + (z^3 + \dots)y=0,$$

which gives $y = R(z)e^z$. If we put $x = \frac{1}{z}$, we find no exponential solution.

Consequently the solution of the equation, if it can be obtained under the form we are now considering, must be one of the two expressions,

$$y = R(x)e^{x^2+\frac{1}{x-1}} \text{ and } y = R(x)e^{-x^2+\frac{1}{x-1}}.$$

As a third example, I take the primitive,

$$y = xe^{x^2+\frac{1}{x^2}},$$

and from it the differential equation

$$x^3 \frac{d^2y}{dx^2} + (2x^2+2) \frac{dy}{dx} - (x^3+4x^2+2x-2)y=0.$$

We must evidently here put $y = R(x)e^{\int \mu dx}$, which gives $\mu = \pm 1$. If $x = \frac{1}{z}$, the equation becomes

$$z^4 \frac{d^2y}{dz^2} - 2z^3 \frac{dy}{dz} - (1+4z+2z^2-2z^3)y=0.$$

If we put here $y = R(z)e^{\int dz(\mu+\nu z)}$, employ the formulæ given in the first part of the paper, and equate the coefficients of the highest powers of (z) to zero, we have $\nu^2-2\nu=0$, $2\mu\nu-2\mu=0$, whence $\nu=2$, and $\mu=0$; and y must be one of the two forms $R(x)e^{x^2+\frac{1}{x^2}}$, or $R(x)e^{-x^2+\frac{1}{x^2}}$.

XVII. "Observations with the Great Melbourne Telescope, in a Letter to Prof. Stokes." By A. LE SUEUR. Communicated by Prof. STOKES, Sec. R.S. Received April 18, 1870.

Observatory, Feb. 27.

DEAR SIR,—I have little more definite to tell you with reference to the star η Argûs; thinking that a larger dispersion would be of advantage, I have had a supplementary arrangement added to the spectroscope, by means of which a direct prism may be interposed between the collimator and the usual prism.

With this increased dispersion, the red line keeps its place; the yellow one turns out to be slightly more refrangible than D.

The green lines, which, with the smaller dispersion, were very difficult, now become almost unmanageable; this would seem to throw some doubt on their reality, as mere extra dispersion should have little effect on real lines.

The direct prism being a small one, does not take in the whole of the pencil when condensed to the limits bearable by the collimator; but as the arrangements at my disposal do not in any case admit of utilizing the full condensation, the smallness of the prism has not had any material effect.

On the whole, I am now inclined to think that, with respect to the green lines, the appearance of the spectra is due to a character of light somewhat similar to that of α Orionis, &c.; a spectrum of groups of dark lines, with spaces more or less free between them, producing the effect (when the light is not sufficient to bear a slit fine enough for dark lines) of a spectrum crossed by bright lines.

The behaviour of the red line, however (of the blue one, being less conspicuous, I cannot speak with so much confidence), would lead to the already drawn inference that it is a real hydrogen line.

I have examined other stars of about the same magnitude as η Argûs; in the majority of these there is not even a suspicion of condensation in any part of the spectrum; red stars, R Leporis for instance, give a spectrum not dissimilar to that of η Argûs; but the red line on none of the stars examined is so conspicuous as in η .

The weather since the beginning of this year has been more favourable, so that I am able, by degrees, to increase the amount of work done. The routine work is the review of figured nebulae; as might be expected, the 4 feet gives views considerably different from the C. G. H. drawings; but at present I have nothing worthy of special mention.

The light of the nebulae, as they are taken up for general examination, is analyzed with the prism; of those which have been examined I have yet found none for which it may be certainly said that the light is not of definite refrangibilities.

In irregular nebulae, the bright knots even, which are so distinctly

mottled as to point to a cluster condition, give out, as far as I have yet seen, light which is monochromatic, or nearly so.

Acknowledged clusters, where discrete stars are plainly discernible, are of course excluded; of the nebulosity mixed up with such clusters as 47 Toucan, I cannot speak with certainty; but if the light were monochromatic, I think that (in the case particularized at least) the brilliancy would be sufficient to afford a definite impression.

Would you call Lord Rosse's attention to 1477-78 (general catalogue), of which I enclose a diagram from measured positions? The configuration differs so widely from that given in the Philosophical Transactions, that, with reference to the rotation of the two nebulous stars, it would be interesting to have the evidence of any additional observations made at Parsonstown.

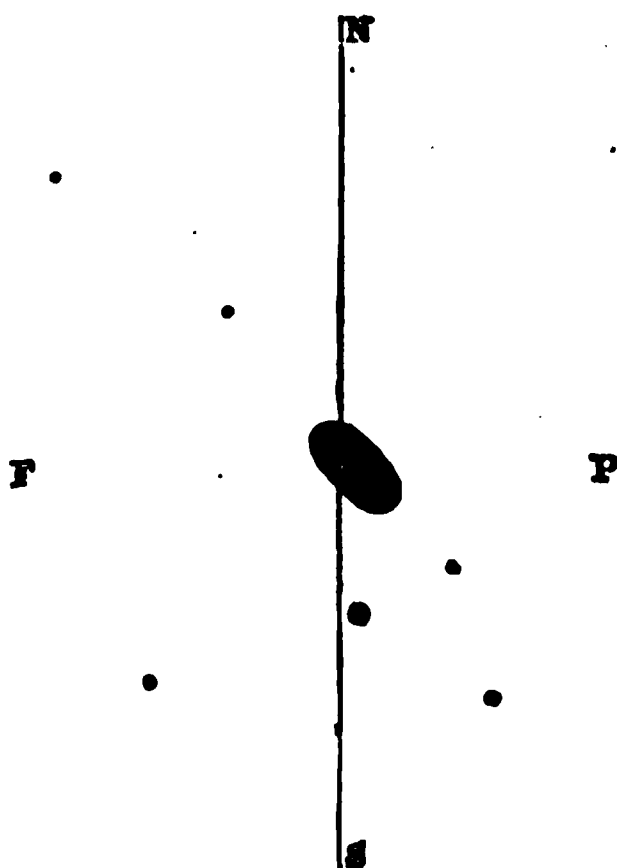
From Mr. Huggins's observations of the nebulae in Orion, I gather that he has seen only the three usual lines; with a wide slit, I had lately a very strong suspicion of a fourth line, probably G. I have not specially examined the nebulae since;

but probably Mr. Huggins will be able to give confirmatory evidence.

On the night of February 1st we had a pretty brilliant auroral display; being at work at the time, I missed part of it; but as soon as I became aware of its existence I applied the spectroscope. At moments four lines already known were easily visible, the chief line being remarkably brilliant. A much narrower slit than that used could have been borne at the time of maximum display, which, however, lasted only a few moments. I was intent on measuring the lines, as at the time I had no published definite information with reference to other than Ångström's special line; but at moments light was seen at the red end of the spectrum sufficiently bright to leave a distinct impression of colour; when, however, special attention was devoted to that part of the spectrum the aurora had greatly diminished in brilliancy, so that I was unable to make out whether a red line existed, or whether there was a general spectrum at the red end. I incline to the latter opinion, and put it down to the rose-coloured arc; this arc, however, which seemed pretty brilliant after the streamers had disappeared, did not *then* give a visible spectrum. Probably this phenomenon has been observed before to better purpose; but I cannot find mention thereof in published accounts.

Yours truly,

A. LE SUEUR.



XVIII. "Chemical and Physiological Experiments on living Cinchonæ." By J. BROUGHTON, B.Sc., F.C.S., Chemist to the Cinchona-Plantations of the Madras Government. Communicated by Dr. EDWARD FRANKLAND. Received May 16, 1870.

(Abstract.)

The memoir describes the principal scientific results which have been obtained during the last three years, in the course of chemical work on the Neilgherry Cinchona-plantations.

The chemical characteristics of the various parts of the Cinchona plant are described. The condition in which the alkaloids are met with in the living bark is shown to be that of a slightly soluble tannate existing in the parenchymatous cells.

The order of formation of the alkaloids is shown to be, 1st, uncrystallizable quinine; 2nd, crystallizable quinine; 3rd, cinchonidine and cinchonine. Reasons are adduced for thinking that the alkaloids are really formed in the tissues in which they are found.

The effect of the solar rays falling on the bark, either while living on the tree or when separated, is shown to be prejudicial to its contained alkaloids. The effects of shielding the bark artificially, and the influence of elevation of the site of growth, are detailed.

The question as to whether the alkaloids are substitutes for the mineral bases is discussed, and a series of experiments is described, which combine to show either that such substitution does not take place, or does so only in a very partial degree.

XIX. "Researches on the Hydrocarbons of the Series $C_n H_{2n+2}$."—VI. By C. SCHORLEMMER. Communicated by Prof. STOKES, Sec.R.S. Received June 14, 1870.

In my last communication* I stated that, from the results of my experiments, I came to the conclusion that, by acting on these hydrocarbons with chlorine, a mixture of *primary* and *secondary* chlorides was formed, as the alcohols derived from these chlorides yielded on oxidation, besides an *acid containing the same number of carbon atoms* as the alcohol, also *acetones*, or the characteristic oxidation products of secondary alcohols.

The correctness of this conclusion has been fully proved by further experiments, and I am at present engaged in investigating the conditions under which the one or the other of these chlorides is formed.

In order to obtain decisive results, it was first of all required to work on considerable quantities of a hydrocarbon; and I selected for this research *hexyl-hydride*, $C_6 H_{14}$, from petroleum, as this body can be obtained the most easily in a sufficient quantity.

The derivatives of hexyl-hydride have been fully investigated by Cahours

* Proc. Roy. Soc. vol. xviii. p. 25.

and Pelouze*, but the results which I have so far obtained differ in several important points from those of these eminent chemists.

By acting on this hydrocarbon with chlorine in the cold and in presence of iodine, I prepared hexyl-chloride, $C_6H_{13}Cl$, already described by Cahours and Pelouze, which boils at 125° – 126° C.; but besides this compound there was also formed a pretty large quantity of a product which distilled between 126° and 135° C., and from which no substance having a constant boiling-point could be isolated. This higher boiling portion can be separated only with difficulty from the chloride boiling at 125° – 126° , for even after repeated distillations a residue of the former is always left behind.

On heating the lower boiling chloride with glacial acetic acid and potassium acetate a hexyl-acetate was obtained, of which the larger portion distilled between 158° and 162° , the boiling-point rising up at last to 170° . Besides the acetate a pretty large quantity of *hexylene*, C_6H_{12} , had also been formed by the decomposition of the chloride.

The chloride boiling between 126° and 135° , treated in the same way, yielded also *hexylene*, besides a hexyl-acetate distilling between 160° and 170° . According to Cahours and Pelouze, this ether boils at 145° .

I did not try to isolate definite compounds from these acetates, as I found that by converting them into the alcohols and subjecting these to fractional distillation, they easily split up into two distinct compounds,—one, which forms the greater part of the mixture, boiling constantly at 140° – 141° , and the other and smaller portion distilling between 150° and 155° ; the quantity of liquid coming over between these two limits being quite insignificant.

The liquid boiling at 140° is a *secondary hexyl-alcohol*; on oxidation it yields first an *acetone*, which, by further oxidation, splits up into *acetic acid* and *butyric acid*. It is therefore *methyl-butylcarbinol*,—



Whether this compound is identical or not with the secondary hexyl-alcohol, which Erlenmeyer and Wanklyn obtained from mannite, I am not yet able to decide.

The body which distilled between 150° and 155° is a *primary hexyl-alcohol*, $C_6H_{13}OH$; on oxidizing it an oily acid was formed, which, as the analysis of its silver-salt showed, has the composition of *caproic acid*, $C_6H_{12}O_2$. Cahours and Pelouze mention in their memoir only the latter alcohol; they do not state, however, under what conditions they acted upon the hydride with chlorine.

I have found that, by treating this hydrocarbon with chlorine alone in the cold as well as at the boiling-point, chlorides are obtained which boil between 125° and 135° , and which appear to be identical with those described above. I intend not only to study these chlorides more fully, but also to

* Annal. Chim. Phys. (4) i. 5.

compare the alcohols obtained from them with the secondary alcohol from mannite and from hexylene, and with the primary hexyl-alcohol which is found in fusel-oil.

XX. "Formation of Cetyl-alcohol by a singular reaction." By C. SCHORLEMMER. Communicated by Prof. STOKES, Sec. R.S. Received June 14, 1870.

On heating a mixture of sebacic acid, $C_{10}H_{18}O_4$, and caustic baryta, besides the hydrocarbon C_8H_{18} , which I have described in a former communication*, other products are formed, amongst which there is a solid body, which, by several crystallizations from alcohol, was obtained in small white crystals.

On analyzing it, Mr. Dearden obtained results which led to the formula $C_{16}H_{34}O$, which is that of *cetyl-alcohol*:—

	Calculated.		Found.	
			I.	II.
C_{16}	192	79·34	79·3	78·9
H_{34}	34	14·05	13·8	13·9
O	16	6·61	—	—
	242	100·00		

This body has not only the composition but also the characteristic properties of cetyl-alcohol; it melts at 49° , and solidifies again at the same temperature.

The formation of this compound is certainly very singular, and perhaps the more so as cetyl-alcohol is so easily oxidized to sebacic acid by the action of nitric acid. I intend to obtain larger quantities of it by the above reaction and to investigate it.

XXI. "Researches in Animal Electricity." By C. B. RADCLIFFE, M.D. Communicated by CHARLES BROOKE, M.A. Received May 19, 1870.

(Abstract.)

PART I.

The subjects of the present inquiry are three in number:—1. The electrical phenomena belonging to living nerve and muscle during rest; 2. The electrical phenomena which mark the passing of nerve and muscle from the state of rest into that of action; and 3. The workings of voltaic electricity, and of electricity generally, upon nerve and muscle.

1. *The electrical phenomena belonging to living nerve and muscle during the state of rest.*

Argument.—Living nerve and muscle have an electricity of their own, which fails by degrees as life dies out, and is wanting altogether after

* Proc. Roy. Soc. vol. xvi. p. 376.

death. This electricity is made known by the electrometer, as well as by the galvanometer. Living nerve and muscle supply to the galvanometer currents, called respectively the *nerve-current* and the *muscle-current*, when the sides of the fibres are connected, through the coil, with either one of the two ends, or when certain points upon the sides or upon the ends are brought together in the same manner, the direction of these currents showing that the sides of the fibres are positive in relation to either of the two ends, or else the reverse (the instances of reversal being the exception and not the rule), and that the positive surface becomes more positive, and the negative surface more negative, as the distance from the line of junction between these surfaces increases. Living nerve and muscle are also (as is now for the first time distinctly proved by means of Thomson's New Quadrant Electrometer) capable of acting upon the electrometer, the action showing that the electrical differences upon which the nerve-current and muscle-current depend are not the same in all parts of the fibres, the differences between the sides and the ends being differences in kind, like those which belong to the two surfaces of a charged Leyden jar, the differences upon the sides singly, and upon the ends singly, being only those which indicate different degrees of tension in one kind of electricity. In accounting for these phenomena, the very imperfect conductivity of nerve and muscle, and of animal tissue generally, is taken as a starting-point. It is assumed (and in support of this assumption some new measurements of the resistance of nerve and muscle to electrical conduction are given) that in nerve and muscle the *sheaths* of the fibres may conduct electricity so imperfectly as to be capable of acting as *dielectrics*,—that a charge of one kind of electricity, developed on their outsides (by oxygenation or in some other way), may *induce* an opposite charge on their insides,—and that the electrical condition of the two ends of the fibres may be opposed to that of the sides, because the charge induced within the sheath is conducted to the ends by the contents of the sheath. It is supposed, in short, that the fibres of living nerve and muscle during rest are so many charged Leyden jars, their electrical condition at this time being statical, not current, and that the nerve-current and muscle-current are no more than accidental phenomena arising from the galvanometer being placed between two points which happen to be electrically dissimilar. And in support of this view it is pointed out that precisely parallel electrical phenomena may be obtained from a piece of wood, shaped like the piece of nerve or muscle, and coated on its sides, but not at its two ends, by a sheath formed of two layers of tinfoil separated by an intermediate layer of thin gutta-percha sheeting, if only the sides be charged as the sides of the piece of nerve or muscle are supposed to be charged, and if the electrodes of the galvanometer or electrometer be applied in the proper manner.

2. *The electrical phenomena which mark the passing of nerve and muscle from the state of rest into that of action.*

Argument.—The nerve-current and muscle-current disappear almost

entirely when nerve and muscle pass from the state of rest into that of action. The "secondary contraction" set up in a muscle by simply laying its nerve upon another muscle or nerve in which a state of action is present, points to a disturbance outside the acting nerve and muscle such as might be caused by a discharge of electricity, and suggests the idea that the sudden disappearance of nerve-current and muscle-current in action may be owing to such discharge; and this view is not a little borne out by certain close anatomical and physiological analogies which are found to exist between the muscular apparatus and the electric organs of the Torpedo. In short, the evidence seems to show that a discharge analogous to that of the Torpedo is developed, as Matteucci supposed, when nerve and muscle pass from the state of rest into that of action, and that the discharge of the Torpedo itself may be nothing more than the unmasked manifestation of a discharge which occurs in a masked form in every case of nervous and muscular action.

3. *The workings of voltaic electricity, and of electricity generally, upon nerve and muscle.*

Argument.—The behaviour of muscle under the action of the so-called "inverse" and "direct" currents is taken as the text in the present inquiry.

Contraction in this case plainly belongs, not to the time when the circuit remains closed, but to the moments [of closing and opening the circuit, when the nerves and muscles are acted upon by instantaneous currents, called *extra-currents*, which currents are in very deed discharges. These extra-currents agree with ordinary induced currents in their discharge-like character; but they disagree in their direction, the extra-current at the closing of the circuit taking the same course as the constant current, the extra-current at the opening having the opposite course; and this point of difference is not to be lost sight of. At first both extra-currents cause contraction; afterwards, when the muscle and nerve have lost some of their susceptibility to impressions, only that extra-current causes contraction which happens to pass in the same direction as that in which motor impulses are transmitted along the motor nerves to the muscles. With this clue, indeed, it is not difficult to trace to its cause every variation in the order of contraction which characterizes the case in question.

Nor is it altogether unintelligible that the behaviour of the muscles as to the continuance of these contractions should, under ordinary circumstances, differ in the case where the current is inverse, and in the case where the current is direct. This difference is noticed when the voltaic circuit is insulated, but not when an earth-wire is put to either of the poles. With the voltaic circuit insulated, the contractions continue for 60' or longer in the case where the current is inverse, and for no longer than 15' or 20' in the case where the current is direct: with the earth-wire at the negative pole the contractions continue for 60' or longer in the case where the current is inverse, and in that in which the current is direct also; with the earth-

wire at the positive pole the contractions continue no longer than 15' or 20' in the case where the current is direct, and in that in which the current is inverse also. With the earth-wire at either pole—that is to say, the part acted upon by the inverse current and the part acted upon by the direct current are both made to contract for the same length of time, the contraction in both parts being 60' or longer if the wire be at the negative pole, and for no longer than 15' or 20' if it be at the positive pole. Now the earth-wire changes the charge of free electricity associated with the inverse and direct currents, but it does not alter the course of those currents. When the voltaic circuit is insulated, the part acted upon by the inverse current is charged positively, and that acted upon by direct current negatively, the charge in each case proceeding from the voltaic pole which happens to be nearest; when the earth-wire is put to either pole, the free electricity of that particular pole runs off to earth, and the parts between the poles (the half traversed by the inverse current and the half traversed by the direct current alike) are charged with the free electricity of the other pole,—with positive electricity if the wire be at the negative pole, with negative electricity if it be at the positive pole. The whole case, indeed, is one which seems to admit of only one conclusion, namely this—that the longer or shorter continuance of the contraction must have its explanation, not in the current being inverse in the one case and direct in the other, but in the free electricity associated with one or both these currents being positive in the one case and negative in the other, the contraction continuing for the longer time when this electricity is positive, and for the shorter time when it is negative. And that this should be so is not altogether unintelligible if the natural electrical condition of the fibres of living nerve and muscle be what it has been assumed to be—a condition in which the outsides and insides of the sheaths are in opposite electrical states, the charge on the outside, usually positive, inducing the opposite charge on the inside; for on this assumption it may well be that a positive artificial charge to the outsides of the sheaths may preserve the natural activity of the fibres, and so favour the continuance of the contraction by keeping up their natural charge, the positive electricity outside the sheaths inducing negative electricity inside the sheaths; and that a negative artificial charge may have the contrary effect, the negative charge outside the sheaths inducing positive electricity within the sheaths, and so producing that reversal in the relative position of the two electricities which is only met with when the fibres are upon the point of losing their activity.

Voltaic electricity, therefore, would seem to act upon nerve and muscle, not by the constant current which passes while the circuit is closed, but by the charge of free electricity, positive or negative, associated with this current, and by the extra-currents which pass at the moments of closing and opening the circuit, which extra-currents are in very deed discharges, the charge being favourable to the continuance of activity when positive, and unfavourable when negative, the instantaneous currents or discharges

causing action. As with the natural electricity of nerve and muscle, so in this case, rest and charge, and action and discharge would seem to go together.

And so also with the action of Franklinic and Faradaic electricity upon nerve and muscle. With Franklinic electricity the state of rest in both nerve and muscle is plainly connected with the charge, and the state of action with the discharge. With Franklinic electricity, too, the positive charge is found to be favourable to the continuance of the state of action, and the negative charge unfavourable. And so likewise with Faradaic electricity, not only as regards the connexion of the state of action with the discharge, for the induced currents may be resolved into discharges, but also as regards the connexion of the state of rest with the charge, for in the interval between the two induced currents the secondary circuit is in fact occupied by a charge of electricity.

PART II.—*On Electrotonus.*

Argument.—There is reason to believe that the whole truth has not yet been elicited respecting the movements of the needle of the galvanometer and the modifications of the activity of the nerve which are characteristic of electrotonus.

The movements of the needle of the galvanometer characterizing electrotonus appear to be due, not, as is commonly supposed, to modifications of the nerve-current consequent upon the action of the voltaic current, but to the passage through the coil of the galvanometer of streams of free electricity, positive or negative, as the case may be, from the voltaic pole which happens to be nearest to the coil,—of free positive electricity from the positive pole in anelectrotonus, of free negative electricity from the negative pole in cathelectrotonus. They cannot, so it is argued, be due to modifications of the nerve-current consequent upon the action of the voltaic current, because the same movements continue when there is no nerve-current to be thus modified, as when a dead nerve is used in place of a living nerve, or even when other bodies are substituted for nerve; they may, so it is suggested, be due to streams of free electricity passing through the coil of the galvanometer from the nearest voltaic pole, because such streams do pass in this direction, and because streams of free electricity from a frictional machine so passed give rise to similar movements,—the stream of positive electricity to the movement of anelectrotonus, the stream of negative electricity to that of cathelectrotonus. This is the view taken of the movements of the needle of the galvanometer characterizing electrotonus.

A different conclusion to that commonly held is also thought to be necessary respecting the modifications of the activity of the nerve in electrotonus. Instead of this activity being suspended in anelectrotonus and exalted in cathelectrotonus, the facts, many of them new, are, when fully realized, found to show that this suspension is met with, not in anelectro-

tonus only, but in cathelectrotonus also. It would seem, indeed, that the only difference between anelectrotonus and cathelectrotonus in this respect is, that this suspension is a little less complete in cathelectrotonus than in anelectrotonus, a lesser "stimulus" serving to cause action in the former state than in the latter. It would even seem that any proper exaltation of activity is to be met with in anelectrotonus rather than in cathelectrotonus. Such are the conclusions respecting the modifications of the activity of the nerve in electrotonus which are believed to be warranted by all the facts, old and new alike.

Nor is the increase of contraction detected by the myograph in cathelectrotonus a sufficient reason for concluding that the irritability of the nerve and muscle is exalted in this state; on the contrary, this increase may be nothing more than the natural consequence of the altered electrical condition in cathelectrotonus. In ordinary muscular action, the state of elongation or relaxation is believed to be caused by the mutual attraction of the charges of opposite electricities disposed upon the two surfaces of the sheaths of the muscular fibres, this attraction compressing the sheaths at right angles to their surfaces; in ordinary muscular action the state of contraction is believed to be brought about by the discharge of the charges which caused the opposite state of elongation, this discharge leaving the fibres free to obey, as simple elastic bodies, the attractive force inherent in the physical constitution of their molecules. In cathelectrotonic muscular action, on the other hand, it is believed that the state of elongation may be greater than that which is natural to the fibres (after removal from the body, at least), because the charge communicated from the negative pole to the fibres is greater than the natural charge of the fibres, the artificial charge to the outside of the sheaths inducing an equivalent charge of the opposite electricity on the insides, and so causing increased elongation by increasing the compression to which the sheaths are subjected between these two charges; and that the contraction may be increased, because contraction, according to this view, is only the return of the fibres, by virtue of their elasticity, from the previous state of increased elongation. The case supposed is precisely that which may be imitated in every particular upon a narrow band of thin india-rubber sheeting, coated with gold-leaf on its two surfaces within a short distance of their edge, or else wetted to the same extent simply with water, and by charging and discharging in turn; for as the charge is communicated the band goes on elongating until the charge has reached its maximum, and when discharge is brought about there is sudden shortening, the degree of shortening being always commensurate with the previous degree of elongation. What happens is that which is supposed to happen in ordinary muscular action and in cathelectrotonic muscular action also, if only the effects of the smaller charge and discharge be made to stand for the first, and those of the fuller charge and discharge for the last form of muscular action. It is of no moment, also, whether the electricity used in charging be negative or positive. Whether

the charge be negative or positive, the results are the same, and therefore it is plain that there ought also to be increased contraction in anelectrotonus if this be the true explanation of the increased contraction which happens in cathelectrotonus. In cathelectrotonus it is assumed that the negative charge from the negative voltaic pole charges the outsides of the sheaths of the fibres negatively, and induces an equivalent charge of positive electricity on the insides; in anelectrotonus, on the other hand, it is assumed that the positive charge from the positive voltaic pole brings about a contrary state of things in the fibres, charging the outsides of the sheaths positively, and affecting the insides negatively by induction. The difference assumed to exist between the two electrotonic states is in the relative position of the two charges upon the sheaths of the fibres, nothing else. It is not a difference which can affect the elongation of the fibre if elongation be brought about by the mutual attraction of the opposite charges with which the sheaths are charged; for the attraction of either charge for the other must be the same, whether it be exercised from within the sheath or from without it. It follows, indeed, from what is supposed, not only that there should be increased contraction in anelectrotonus as well as in cathelectrotonus, but also that the state of rest in both electrotonic conditions should be characterized by increased elongation. And what there should be in theory there is in fact; for it proves on inquiry that contraction may be caused in anelectrotonus by an adequate "stimulus," that this contraction is greater than that caused by the same "stimulus" in the unelectrotonized state, and that actual increased elongation of the fibres is an effect of both cathelectrotonus and anelectrotonus. The view of muscular action here taken is that which has been always advocated by the author as regards contraction, but it is modified somewhat as regards elongation; for now, instead of looking upon elongation as arising from the mutual repulsion among the muscular molecules set up by the presence in the muscle of a single charge of electricity, this state is referred to the mutual attraction of opposite electrical charges disposed, as in a Leyden jar, upon the two surfaces of the sheaths of the muscular fibres.

Looking back, then, at the history of electrotonus there appears to be nothing contradictory to what has been already said respecting the workings of electricity upon nerve and muscle. It is still the same story of rest along with the state of charge, and of action along with the state of discharge, with this significant addition, that in electrotonus the charge is shown, not only as coincident with the state of rest, but as having an actual power of suspending action in both nerve and muscle, and of causing increased elongation of the fibres in muscle.

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The Society adjourned over the Long Vacation to Thursday, November 17.

"On Approach caused by Vibration." By FREDERICK GUTHRIE, B.A. Communicated by Prof. G. G. STOKES, Sec. R.S. Received August 26, 1869*.

§ 1. The chain of experiments which I have to describe arose from the endeavour to explain an observation that a delicately suspended piece of cardboard moves, from a considerable distance, towards a vibrating tuning-fork. It will be preferable to detail the experiments, not in the order in which they occurred to me and were actually performed, but in the order in which I conceive them to form a logical sequence.

§ 2. The experiment of Clement shows that when a continuously renewed current of air passes between two parallel disks from the common axis towards the circumference, the disks are urged together. Consequently, in seeking to explain the fact observed in § 1, it was necessary to examine the air surrounding the resonant fork in order to ascertain whether air-currents existed in its neighbourhood; and further, to distinguish between such currents as might be found to move in closed curves forming whirlwinds in the immediate neighbourhood of the fork, and such as might radiate in unclosed paths from the fork through the air.

§ 3. In 1831 Mr. Faraday†, in tracing the cause of the accumulation of light particles on the internodal points and lines of vibrating bodies, came to the conclusion that such accumulation was due to minute whirlwinds, and not, as had been held by M. Savart‡, to the existence of secondary nodes. A general conclusion at which Mr. Faraday arrived was this: whenever the different parts of a surface are vibrated to different degrees, there is always a tendency for the air to flow along the surface of the vibrating body towards the more violently agitated portions from the less agitated.

§ 4. It is clear that, before examining the possible connexion between these superficial whirlwinds and the fact mentioned in § 1, it is necessary to examine into the existence of air-currents of unclosed paths.

* Read Dec. 17, 1868. See abstract, vol. xvii. p. 106.

† Phil. Trans. 1831, p. 290.

‡ Ann. de Chim. et de Phys. t. xxxvi. pp. 167, 267.

The tuning-fork which was most employed, and which I call fork A, gave 128 complete vibrations per second, and had the following dimensions :—

mètre.

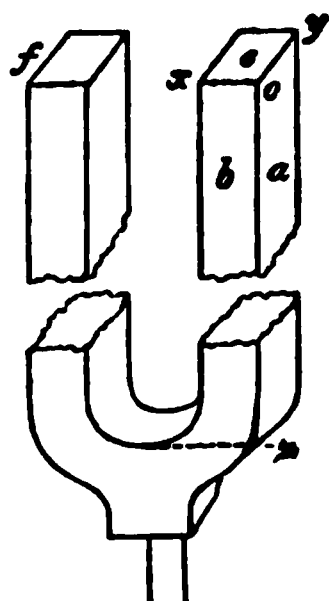
$$oy = 0.0230,$$

$$ox = 0.0172,$$

$$oz = 0.3255.$$

I call the three faces intersecting in o , the faces a , b , and c respectively. Let the symbol H_c , &c. denote the position of the fork when the face c is horizontal, &c.

Fig. 1.



§ 5. *Experiment 1.*—The fork A was set vibrating by drawing the bow across the edge oy ; the plane of vibration was accordingly parallel to b . The fork was then brought into the neighbourhood of an ascending thread of smoke. The fork and smoke had in succession the three relative positions :—

(1) H_b . The smoke passed across the face a parallel to oy .

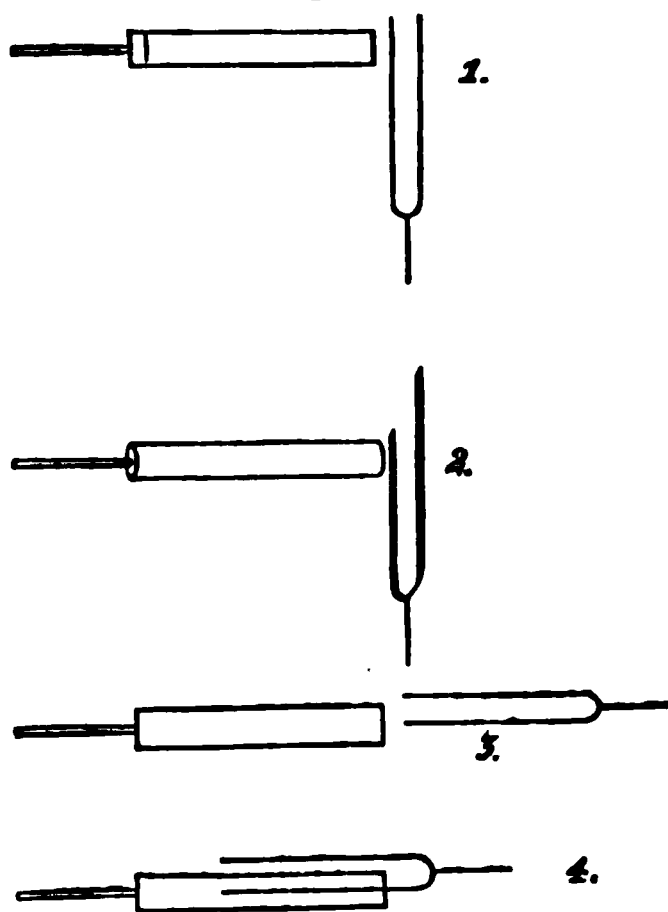
(2) H_a . The smoke passed across the face b parallel to ox .

(3) H_c . The smoke passed across the face c parallel to ox .

In all cases the smoke clung to the surface across which it passed as though the fork were at rest.

§ 6. *Experiment 2.*—A cylindrical glass tube T, 0.4 m. long and 0.042 m. in internal diameter, was fastened (fig. 2) in a horizontal position. One end was left open, the other carried a cork, through the centre of which passed a horizontal tube t , 0.04 m. long and 0.0035 m. in internal diameter. These tubes were filled with smoke, and the fork A, which had been set vibrating as in § 5, was brought to the open end of the wide tube. The fork and tube had in succession the four relative positions shown in fig. 2, namely :—

Fig. 2.



(1) H_c . Axis of tube perpendicular to a .

(2) H_c . Axis of tube perpendicular to b .

(3) H_a or H_b . Axis of tube perpendicular to c .

(4) The same as (3), but having one prong of the fork thrust as far as possible into the tube T. In none of the cases did the smoke show any tendency to escape through the tube t , nor was fresh air drawn in.

§ 7. *Experiment 3.*—The cork and tube t of experiment 2 were with-

drawn and replaced by a film-bubble of glycerine-soap-water. The combinations of position of experiment 2 were repeated. In none of the cases did the bubble show any variation from the vertical plane.

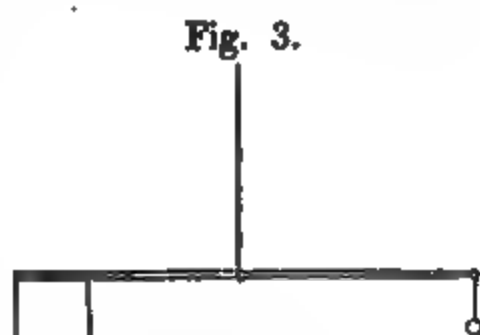
§ 8. Hence I conclude that when a tuning-fork is in a state of plane vibration, no permanent true air-currents are formed; that is, no air-currents could be detected departing from any side of the fork and penetrating the surrounding air in unclosed paths.

§ 9. The superficial whirlwinds examined by Mr. Faraday may be supposed to be greatly modified when they are excited in the immediate neighbourhood of a solid body; and as the "attraction" which formed the starting-point of the present examination (§ 1) is exerted upon a solid body in the neighbourhood of the resonant fork, some experiments, supplementary to those of Mr. Faraday, were found necessary.

§ 10. Numerous experiments, which need not be here detailed, showed (1) that Mr. Faraday's surface-currents, as exhibited on a freely vibrating fork, are very much modified when the fork vibrates in the immediate neighbourhood of a rigid plane, and (2) that the effects of any currents produced by vibration do not extend sensibly beyond 0.006 m. from the fork's face, and only even to this extent near the *a* face.

§ 11. We shall see that the existence of such air-circuits, confined as they are to the immediate vicinity of the fork, are quite insufficient to account for the class of phenomena which have to be described, and which are similar to the fundamental fact mentioned in § 1.

§ 12. *Experiment 4.*—To one end of a splinter of wood, 0.5 m. long, a card 0.08 m. square was fastened in such a way that the plane of the card was vertical, and contained the line of the splinter. The whole was hung from a fibre of unspun silk (fig. 3) and counterpoised. The tuning-fork A was set in vibration as before, and was brought towards the card in the three relative positions corresponding to those of § 6, namely:—



(1) (H_c). The face *a* parallel to the card.

(2) (H_c). The face *b* parallel to the card.

(3) (H_a or H_b). The face *c* parallel to the card.

In all three cases the card moved towards the fork. The rate at which the card moved was greatest when the fork was sounding loudest. In all three cases it was possible to draw the card from a distance of 0.05 m. at least,—a distance quite beyond the direct influence of the superficial whirls which exist in position (1) (on face *a*).

§ 13. There is perhaps nothing essentially contrary to reason in the conception of two bodies in space free to move, so related to one another that while the first has no tendency to move towards the second, the second has a tendency to move towards the first. But if the tendency of the one

to move be caused by the condition of the medium between the two, it seems inevitable that the tendency shall be mutual. Thus, if that tendency result from a general diminution in the tension of an elastic medium between the two, they will be urged towards one another. To test the reciprocity of the motive tendency in the case under consideration the following experiment was tried.

§ 14. *Experiment 5.*—The tuning-fork A was fastened to the end of a rod 1.0 m. long; the other end of the rod was counterpoised, and the whole was hung from a silk tape. If the vertical plane passing through the rod be called V, then the rod and fork received in succession the relative positions,—

- (1) (H_a). V parallel to a .
- (2) (H_b). V parallel to b .
- (3) (H_a or H_b). V parallel to c .

In (1) and (2) the fork was simply hung from the suspended rod; in (3) it was fastened to an iron rod in the direction of its axis, and the two were then attached to the suspended rod at their common centre of gravity. The fork was sounded by the bow as before, and a piece of card 0.05 m. square was brought near the face a (in 1), b (in 2), and c (in 3). In all cases the suspended fork approached the card; but, owing to the great inertia of the suspended fork and counterpoise, the motion was much slower and less striking than was the case when the card was hung.

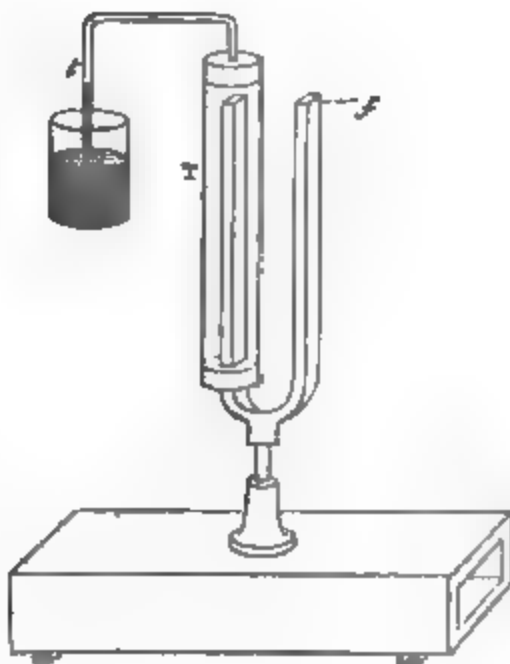
§ 15. *Experiment 6.*—Further, instead of a card, a second fork, B (sounding A), was set in vibration, and brought into the neighbourhood of the vibrating suspended fork A. The three faces a' , b' , c' of the fork B were held in succession parallel to the three faces a , b , c of the fork A, that is parallel to V when the faces a , b , c were in each of the three positions described in § 14. There were thus nine combinations effected. In every case the suspended fork approached the stationary one. Hence, to whatever cause the approach is due, the action is mutual.

§ 16. The next question, the solution of which promised to throw light upon our problem, was this: What is the general or mean condition as to tension of a medium in which undulations are generated? Though this question has received very great attention from theoretical physicists, it has not been approached, as far as I am aware, from the side of experiment in the manner to be described.

§ 17. *Experiment 7.*—The fork A was fixed in an upright position in its sounding-box (fig. 4). One of its prongs was enclosed in a glass tube T, 0.4 m. long, and 0.042 m. internal diameter, carrying a cork through which the prong passed. The upper end of T also carried a cork, through which passed a narrow tube t , bent twice at right angles, and dipping into water. The internal diameter of t was 0.0035 m. The corks of the tube T were made tight with wax, and a little air was expelled from the tube T by warming it with the hand, so that when the atmospheric temperature was regained, the water stood at some distance up the tube t . The tube t

was firmly clamped in several places to prevent vibration, and consequent centrifugal effect. On passing the bow across f , the enclosed prong was also set in vibration. When the amplitude of the vibration was as great as possible, the water had sunk in the tube t to the amount of 0.003 m. The moment both prongs were suddenly stopped the level of the water in t was restored. The depression of the water in t cannot be due to increased temperature; for, if it were so, the increase of volume would be gradual and accumulative, and, on stopping the vibration, the contraction due to cooling would be also gradual; whereas the attainment of maximum depression and the restoration to normal volume are practically instantaneous.

Fig. 4.



§ 18. We have here accordingly an experimental proof that the rapid motion (in this instance vibration) of a body in a medium produces on the whole an effect similar to that which would be produced by the expansion of the body, namely, a displacement of the medium. If air were perfectly elastic and had no inertia, no such total displacement could ensue, and I think I may safely predict that the apparent expansion of the medium will be found, in the case of hydrogen less, and in the case of carbonic acid greater than in that of air*.

§ 19. Though we know the dimensions of the fork and its rate of vibration, and though we can measure with tolerable accuracy the amplitude of its vibrations, we can only calculate from this the mean velocity of any given point, because in the middle of a vibration the fork is moving very much faster than towards the commencement or termination. Hence this vibratory displacement cannot, with our present data, be connected with the known rate at which air enters a vacuum.

§ 20. The fundamental experiment of §§ 1, 12 next suggested for its explanation the following question. Let there be two equal and opposite forces, P and Q , producing equilibrium upon a body having inertia; let one of them, P , be increased and diminished by a series of equal increments and decrements following one another in rapid succession. Will the continually varying force, whose mean is P , maintain average equilibrium with the unaltered force Q ? The plane of the cardboard in § 1 and § 12 is the seat of two opposing forces, namely the pressure of the atmosphere on both sides. When the sounding-fork is held on one side, the pressure on that side undergoes successive equal increments and decrements. Accordingly, if the question just proposed be answered in the negative, a suffi-

* Compare the sighing of an organ pipe after it has been sounded.

cient ground would be at hand for the approach of the cardboard to the fork.

§ 21. *Experiment 8.*—A “Cartesian diver” was made out of a test-tube, a bubble of air, and a beaker-glass of water. This was so nicely adjusted that it rose when near the surface of the water, and sank when the top of the tube was 0·05 m. below the surface. When resting on the bottom of the beaker, the top of the test-tube was 0·067 m. below the surface of the water. When the diver was resting on the bottom of the beaker, the tuning-fork A, in a state of vibration, was presented to the glass in various directions with regard to the tube. The fork was placed sometimes in contact with the water, sometimes in the neighbouring air, and sometimes in contact (towards the base of the fork) with the glass. Although the vibration of the bottom of the beaker caused the diver to leap up it invariably sank again, and showed no sign of undergoing any alteration in specific gravity. If, now, the question in § 20 were answerable in the negative, the equilibrium would have been destroyed, because the atmospheric pressure on the one hand, and the elasticity of the confined air on the other being equal and opposite forces, an alteration in one caused by its subjection to successive sonorous waves, would have altered the volume of the confined air and so destroyed the equilibrium.

§ 22. I hoped to throw light upon the fundamental experiment of § 1 and § 12, by varying the nature of the surface of the body which received the vibrations, with the view on the one hand of preserving them, and, on the other, of dispersing them as much as possible. With this view experiments 9–12 were undertaken.

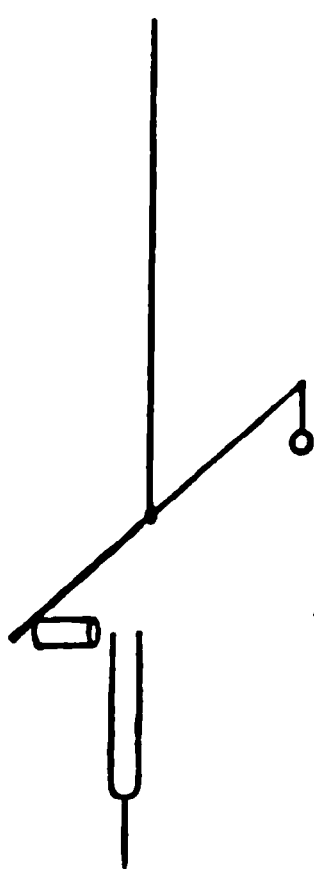
§ 23. *Experiment 9, fig. 5.*—Upon one end of a splinter of wood 0·5 m. long, a cylinder of cardboard 0·03 m. in diameter and 0·04 m. deep, closed at the bottom, was fastened in such a manner that its axis was horizontal, and its bottom in the plane V. The cylinder was counterpoised, and the whole was hung from an unspun silk thread. The vibrating-fork A was brought near the open end of the cylinder in the three positions already described, and also with one prong inserted into and nearly touching the bottom of the cylinder. In all cases motion towards the fork ensued.

§ 24. *Experiment 10.*—A handful of cotton-wool was hung upon the splinter in place of the cylinder of experiment 12. The cotton-wool moved towards the fork from a distance of at least 0·05 m., when the latter was presented to it in either of the three positions, § 8.

Muslin and washleather behaved in a similar manner.

§ 25. *Experiment 11.*—A circular paper drum 0·25 m. in diameter having a rim 0·025 m. deep, was hung by a silk tape in the same manner as the cylinder of § 23. Parchment was stretched across the wide end of

Fig. 5.



a funnel 0·2 m. in diameter. The neck of the funnel was placed in the mouth, and the drum of the funnel was brought opposite and parallel to the edged face of the paper drum. Air was rapidly forced into and drawn out of the funnel. The paper drum moved towards the funnel even from a distance of 0·1 m.

§ 26. *Experiment 12.*—A sheet of cardboard 0·4 m. square was hung in the plane V from a rod 1·0 m. long. The cardboard was counterpoised, and hung from a silk tape. The paper drum of § 25 was placed 0·05 in. from the cardboard, and parallel to it, and was then thipped. The cardboard moved towards the drum.

§ 27. *Experiment 13.*—A rod of brass 1·2 m. long, provided at the ends with disks of brass perpendicular to the rod 0·26 m. in diameter, was set in longitudinal vibration by means of resined leather. One of the disks was held, during the vibration, near to the cardboard of § 26, also near the cotton-wool and muslin of § 24. In all cases the suspended body moved towards the disk. By this means it was easy to cause motion when the two were at the distance of 0·2 m.

§ 28. I have in the foregoing paragraphs sought to eliminate systematically secondary and disturbing influences from the fundamental experiment: The experimental results appear to me to point to the following conclusions.

Whenever an elastic medium is between two vibrating bodies, or between a vibrating body and one at rest, and when the vibrations are dispersed in consequence of their impact on one or both of the bodies, the bodies will be urged together.

The dispersion of a vibration produces a similar effect to that produced by the dispersion of the air-current in Clement's experiment, and, like the latter, the effect is due to the pressure exerted by the medium, which is in a state of higher mean tension on the side of the body furthest from the origin of vibration than on the side towards it.

In mechanics,—in nature there is no such thing as a pulling force—though the term attraction may have been used in the above to denote the tendency of bodies to approach, the line of conclusions here indicated tends to argue that there is no such thing as attraction in the sense of a pulling force, and that two utterly isolated bodies cannot influence one another.

If the ætherial vibrations which are supposed to constitute radiant heat resemble the aerial vibrations which constitute radiant sound, the heat which all bodies possess, and which they are all supposed to radiate in exchange, will cause all bodies to be urged towards one another.

On Jacobi's Theorem respecting the relative equilibrium of a Revolving Ellipsoid of Fluid, and on Ivory's discussion of the Theorem." By I. TODHUNTER, M.A., F.R.S., late Fellow of St. John's College, Cambridge. Received November 23, 1869*.

1. The late James Ivory contributed to the Philosophical Transactions various memoirs on the subject of the equilibrium of fluids and the figure of the earth: the memoirs will be found in the volumes for 1824, 1831, 1834, and 1839. Ivory objected to the received theory of the equilibrium of fluids, and advocated some peculiar opinions at great length, and with much repetition. I do not propose now to criticise these memoirs; I will merely state that I consider them to be altogether unsatisfactory.

2. There is, however, one theorem in the general subject to which I now propose to draw attention, namely, Jacobi's theorem respecting the possibility of the relative equilibrium of an ellipsoid of fluid having three unequal axes and revolving about the least. Ivory discussed this theorem, and his errors are so numerous and so singular, that I have thought it would be desirable to place the corrections before the Society which originally received and published Ivory's communications. In correcting Ivory's errors and supplying his defects, I shall add something to the discussions which have hitherto been given of the theorem itself. It will be seen as we proceed that one of Ivory's errors has been already noticed and corrected.

3. Ivory first alluded to the matter in the memoir of 1834, which was read to the Royal Society a few months before Jacobi announced his discovery of the theorem. Of course at that date Ivory held the common opinion, that the relative equilibrium of a revolving ellipsoid with three unequal axes was impossible. But he does not merely acquiesce in the erroneous opinion, he attempts to *demonstrate* it in the following manner:—

“Further, the figure of the fluid in equilibrium can be no other than a spheroid of revolution. Draw a plane through the axis of rotation and any point (xyz) in the surface of the fluid. This plane will contain that part of the attraction of the spheroid which is parallel to the axis of rotation, or to the coordinate x : it will also contain the centrifugal force directed at right angles from the axis of rotation. *The same plane will also contain the resultant of the attractions parallel to y and z* ; for if it did not, the resultant might be resolved into two forces, one contained in the plane, and the other perpendicular to it; and the force perpendicular to the plane would partly act in a direction touching the surface of the spheroid, which is inconsistent with the equilibrium of the fluid. Wherefore, the whole attractive force at any point in the surface of the spheroid is contained in a plane passing through the point and the axis of rotation;

* Read Jan. 20, 1870. See vol. xviii. p. 171.

which obviously excludes ellipsoids with three unequal axes, and limits the figures of equilibrium to spheroids formed by the revolution of an ellipse about the axis of rotation ; . . .”

The error here begins with the sentence which I have put in italics ; the resultant of the attractions parallel to y and z need not act in the plane which Ivory specifies : the component which he obtains in a plane touching the surface may be balanced by a like component arising from the attraction parallel to x and the so-called centrifugal force.

4. To the Philosophical Transactions for 1838 Ivory contributed a memoir of ten pages on Jacobi's theorem. Ivory devotes a few sentences to the history of the matter. He records the fact that Lagrange had inferred that the figure of relative equilibrium must be a figure of revolution. He makes no allusion, however, to his own erroneous demonstration in the volume for 1834.

5. The object of the memoir seems to be twofold—to establish Jacobi's theorem, and to deduce numerical results relating to the extreme-possible cases analogous to those which had long been known relating to the extreme possible cases for an ellipsoid of revolution. The first object is attained ; Jacobi's theorem is demonstrated in a manner resembling that which had previously been used by Liouville. The second object Ivory fails to attain, owing to an error in his process.

6. In the second page of the memoir there is an error in mechanics resembling that which we have already noticed in Art. 3. At any point in the surface of an ellipsoid, let the normal to the surface be drawn ; and let it be terminated by the principal plane which is perpendicular to the axis of rotation : let ρ be the length of this straight line. At the same point in the surface draw a straight line in the direction of the resultant of the attraction of the whole mass of the ellipsoid, and let it be terminated by the same principal plane ; let ρ' be the length of this straight line, then Ivory says :—

“ Let σ denote the third side of the triangle which has ρ and ρ' for its other sides : then σ will represent the only force which, together with the attractive force ρ' , will produce a resultant in the direction of ρ at right angles to the surface of the ellipsoid.”

This statement is quite wrong. Any straight line which is in the same plane as the normal at any point, and the direction of the resultant attraction at that point, may be taken for the *direction* of such an additional force as Ivory requires ; and the *magnitude* of the force can then be properly determined :

7. In order to render the discussion of Ivory's memoir readily intelligible, it will be necessary to indicate briefly the demonstration of Jacobi's theorem.

Let the equation to an ellipsoid be

$$x^2 + \frac{y^2}{1+\lambda^2} + \frac{z^2}{1+\mu^2} = k^2.$$

The attractions which the ellipsoid exerts on a point (x, y, z) parallel to the axes of coordinates are known to be respectively Ax, By, Cz , where

$$A = \frac{3M}{k^3} \int_0^1 \frac{u^2 du}{H}, \quad B = \frac{3M}{k^3} \int_0^1 \frac{u^2 du}{(1 + \lambda^2 u^2) H}, \quad C = \frac{3M}{k^3} \int_0^1 \frac{u^2 du}{(1 + \mu^2 u^2) H}.$$

Here M denotes the mass of the ellipsoid, and H stands for

$$\sqrt{(1 + \lambda^2 u^2)(1 + \mu^2 u^2)}:$$

see the '*Mécanique Céleste*,' Livre III. No. 7.

Take the axis of x for that of revolution, and let ω be the angular velocity. Then the necessary and sufficient condition for relative equilibrium is that the equation

$$Axdx + (B - \omega^2)ydy + (C - \omega^2)zdz = 0$$

should coincide with the differential equation to the surface of the ellipsoid, namely,

$$xdx + \frac{ydy}{1 + \lambda^2} + \frac{zdz}{1 + \mu^2} = 0.$$

Hence we have the conditions

$$\frac{B - \omega^2}{A} = \frac{1}{1 + \lambda^2}, \quad \frac{C - \omega^2}{A} = \frac{1}{1 + \mu^2}. \quad \dots \dots \dots (1)$$

If we eliminate ω^2 between these we obtain

$$(1 + \lambda^2)(1 + \mu^2)(B - C) = A(\mu^2 - \lambda^2). \quad \dots \dots \dots (2)$$

But from the values of B and C we have

$$B - C = \frac{3M}{k^3} (\mu^2 - \lambda^2) \int_0^1 \frac{u^4 du}{H^3}.$$

Thus (2) becomes

$$(\mu^2 - \lambda^2) \left\{ (1 + \lambda^2)(1 + \mu^2) \int_0^1 \frac{u^4 du}{H^3} - \int_0^1 \frac{u^2 du}{H} \right\} = 0.$$

This may be satisfied by putting $\mu^2 = \lambda^2$, which gives us an ellipsoid of revolution. Or it may be satisfied by making

$$(1 + \lambda^2)(1 + \mu^2) \int_0^1 \frac{u^4 du}{H^3} = \int_0^1 \frac{u^2 du}{H};$$

this may be reduced to

$$\int_0^1 \frac{u^2(1 - u^2)(1 - \lambda^2 \mu^2 u^2) du}{H^3} = 0. \quad \dots \dots \dots (3)$$

We have then to show that for suitable values of λ and μ this relation will hold. This will be shown in the sequel. We must also show that the value of ω^2 is positive. From (1) we have

$$\frac{B - \omega^2}{C - \omega^2} = \frac{1 + \mu^2}{1 + \lambda^2}$$

so that

$$\omega^2 = \frac{B-C}{\lambda^2 - \mu^2} + \frac{B\lambda^2 - C\mu^2}{\lambda^2 - \mu^2}.$$

Put in the values of B and C, and this reduces to

$$\omega^2 = \frac{3M}{k^2} \int_0^1 \frac{x^2(1-x^2)dx}{H^2},$$

so that ω^2 is positive.

8. Before considering Jacobi's theorem, we will advert to the case in which $\mu^2 = \lambda^2$.

We have from (1)

$$\omega^2 = B - \frac{A}{1 + \lambda^2};$$

let ρ be the density of the ellipsoid, then

$$M = \frac{4\pi\rho k^2(1 + \lambda^2)}{3}.$$

Put q for $\frac{\omega^2}{4\pi\rho}$; thus

$$q = 3l^2 \int_0^1 \frac{x^2(1-x^2)dx}{(1+l^2x^2)^2}, \quad \dots \dots \dots (4)$$

Here we have changed u into x , and λ into l , in order to have the same notation as Ivory has. Ivory says:

"From the equation (4) we learn that q will be known when l is given, or that every spheroid of a determinate form requires an appropriate velocity of rotation.

"The inspection of the same equation is sufficient to show that q is positive for all values of l^2 ; and as it vanishes both when l^2 is zero and infinitely great, it must pass at least once from increasing to decreasing, or it will admit of at least one maximum value. By differentiating with regard to l we obtain

$$\frac{dq}{2ldl} = \int_0^1 \frac{3x^2(1-x^2)(1-l^2x^2)dx}{(1+l^2x^2)^3}, \quad \dots \dots \dots (5)$$

from which formula we learn that $\frac{dq}{2ldl}$ is positive between the limits $l^2=0$ and $l^2=1$; that it will consist of a positive and a negative part when l^2 is greater than 1; and the positive part decreasing *while the negative part increases*, that it will ultimately be negative when l^2 is infinitely great. It follows therefore that $\frac{dq}{2ldl}$ can be only once equal to zero, and consequently that q can have only one maximum value, while l^2 increases from 0 to ∞ ."

This is quite unsound, because the words which I have put in italics are untrue. There are two ways of separating the integral into a positive part and a negative part. We may take for the positive part the integral

between the limits 0 and $\frac{1}{l}$, and for the negative part the integral between the limits $\frac{1}{l}$ and 1. Or we may put the integral in the form

$$3 \int_0^1 \frac{x^2(1-x^2)dx}{(1+l^2x^2)^3} - 3l^2 \int_0^1 \frac{x^4(1-x^2)dx}{(1+l^2x^2)^3}.$$

I do not know which of these two ways Ivory adopted. It is true that in each of them the positive part decreases as l increases; but in each of them the negative part is finite when l is finite, and is infinitesimal when l is infinite, and so does not always increase with l as Ivory supposes.

9. However, although Ivory's reasoning is unsound, the result which he wishes to establish is correct, as I shall now show.

Consider the integral $\int_0^1 \frac{x^2(1-x^2)(1-l^2x^2)dx}{(1+l^2x^2)^3}$. We have to show that as l increases from unity to infinity, the integral vanishes and changes sign once, and *only once*.

It is obvious that the integral must vanish once, because it is positive when $l=1$, and negative when $l=\infty$: it is indeed infinitesimal in the latter case, but the sign is certainly negative.

Put z for lx ; thus the integral becomes

$$\frac{1}{l^5} \int_0^l \frac{z^2(l^2-z^2)(1-z^2)dz}{(1+z^2)^3} = \frac{u}{l^5}, \text{ say.}$$

We have to show that the equation $u=0$ can have only one root between $l=1$ and $l=\infty$. If the equation could have more than one root it must have three roots at least; and then the equation $\frac{du}{dl}=0$ must have two roots at least. Now

$$\frac{du}{dl} = 2l \int_0^l \frac{z^2(1-z^2)dz}{(1+z^2)^3} = 2lv, \text{ say,}$$

and

$$\frac{dv}{dl} = \frac{l^2(1-l^2)}{(1+l^2)^3}.$$

Thus v is positive when $l=1$, and while l increases $\frac{dv}{dl}$ is always negative; thus v continually diminishes algebraically, and so cannot change sign more than once. Hence $\frac{du}{dl}$ cannot vanish more than once; it will vanish once because v is positive when $l=1$, and has a finite negative value when $l=\infty$, namely,

$$\int_0^{\frac{\pi}{2}} \sin^2\theta(1-2\sin^2\theta)d\theta, \text{ that is } -\frac{\pi}{8}.$$

10. There is also another way in which the result may be established. For from (4), by the known method of integration, we have

$$q = \frac{3(3+l^2)}{2l^2} \tan^{-1} l - \frac{9}{2l^2};$$

therefore

$$-\frac{dq}{dl} = \frac{3(9+l^2)}{2l^2} \tan^{-1} l - \frac{3(9+7l^2)}{2(1+l^2)l^2}.$$

Thus $\frac{dq}{dl}$ vanishes when

$$\tan^{-1} l = \frac{9l+7l^3}{(1+l^2)(9+l^2)}. \quad \dots \dots \dots (6)$$

Ivory gives these formulæ with some misprints.

Now it has been shown by Laplace that the equation (6) has one positive root, and only one, namely, when $l=2.529\dots$. See the '*Mécanique Céleste*,' Livre III. No. 20.

11. We now return to Jacobi's theorem. We take the integral in (3), and put x for u ; we put $\lambda-\mu=\tau$, and we may suppose λ greater than μ , so that τ is positive; and we put $\lambda\mu=p$. Thus (3) becomes

$$\int_0^1 \frac{x^2(1-x^2)(1-p^2x^2)dx}{\{(1+px^2)^2+\tau^2x^2\}^{\frac{3}{2}}} = 0. \quad \dots \dots \dots (7)$$

Denote the left-hand member of (7) by V ; then we propose to examine the range of values of p and τ which make V vanish, supposing both p and τ positive. If we regard p as an abscissa, and τ as an ordinate corresponding to p , we in effect propose to trace in the first quadrant the curve determined by the equation $V=0$.

12. If we put $\tau=0$, the equation (7) becomes

$$\int_0^1 \frac{x^2(1-x^2)(1-p^2x^2)dx}{(1+px^2)^3} = 0. \quad \dots \dots \dots (8)$$

Ivory says: "It is obvious that there is only one value of p that will verify the equation just found; for the integral can pass only once from being positive to be negative while p increases from 1 to be infinitely great."

I cannot admit that this assertion is obvious; the result, however, may be established by the following investigation:—

Denote the integral by u ; as long as p is less than unity, u is positive, and when p is infinite u is negative. Thus u must vanish once as p changes from unity to infinity: we have to show that u can vanish only once.

If the equation $u=0$ could have more than one root, it must have three roots at least; and then the equation $\frac{du}{dp}=0$ must have two roots at least; this we shall show to be impossible.

We have

$$\frac{du}{dp} = - \int_0^1 \frac{x^4(1-x^2)(3+2p-p^2x^2)dx}{(1+px^2)^4};$$

it is obvious then that $\frac{du}{dp}$ cannot vanish until $\frac{p^2}{3+2p}$ is greater than unity, that is, until p is greater than 3. Thus $\frac{du}{dp}$ is negative when $p=3$; and we can see that $\frac{du}{dp}$ is positive when $p=\infty$. Hence $\frac{du}{dp}$ changes sign once as p increases from 3 to infinity.

Put $px^2 = \tan^2 \theta$, and let $p = \tan^2 \beta$; thus

$$\frac{du}{dp} = \frac{1}{p^{\frac{3}{2}}} \int_0^{\beta} \frac{\sin^4 \theta}{\cos^2 \theta} (p \cos^2 \theta - \sin^2 \theta) \left\{ 1 - \left(3 + \frac{3}{p} \right) \cos^2 \theta \right\} d\theta = \frac{v}{p^{\frac{3}{2}}}, \text{ say.}$$

Then we know that v is negative when $p=3$, and positive when $p=\infty$. If, then, the equation $v=0$ could have more than one root, it must have three roots at least, and then the equation $\frac{dv}{dp}=0$ must have two roots at least.

Now

$$\frac{dv}{dp} = \int_0^{\beta} \sin^4 \theta \left(1 - 3 \cos^2 \theta - \frac{3 \sin^2 \theta}{p^2} \right) d\theta,$$

$$\frac{d^2v}{dp^2} = \frac{6}{p^3} \int_0^{\beta} \sin^2 \theta d\theta + \frac{(p-3)\sqrt{p}}{2(1+p)^{\frac{3}{2}}}.$$

Thus $\frac{d^2v}{dp^2}$ is always positive when p is greater than 3, and therefore $\frac{dv}{dp}$ continually increases, and so cannot vanish more than once.

Hence v cannot vanish more than once, and therefore u cannot vanish more than once. Thus u is always negative when p has any value greater than that which makes u vanish.

13. There is also another way in which the result may be established. It will be found that

$$\int_0^1 \frac{x^2(1-x^2)(1-p^2x^2)dx}{(1+px^2)^3} = \frac{3+13p}{8p^2} - \frac{3+14p+3p^2}{8p^{\frac{5}{2}}} \tan^{-1} \sqrt{p}.$$

Then it may be shown that the last expression will vanish once, and only once, as p changes from 1 to ∞ . This method is adopted by Liouville in an article in his 'Journal de Mathématiques' for April 1839: the article consists of observations on the memoir by Ivory, which we are discussing.

Liouville says that the value of p , which makes the last expression vanish, is a little less than 2. Ivory has a formula which is equivalent to this, but he does not employ it to show that there is only one value of p ; for he had already, as we have seen, stated this to be obvious. From the circumstance that Liouville gives a strict demonstration, it is plain that he agrees with me in thinking that Ivory's statement is not obvious.

According to Ivory, the value of p which satisfies (8) is 1.9414. . . . We will denote this by p_0 .

14. I shall now show that if we ascribe to p any value greater than p_0 , a corresponding value of r exists, which will make V vanish.

Let such a value be ascribed to p ; then from Art. 12 it follows that, with this value of p and with $\tau=0$, the value of V will be negative: we shall now show that by taking τ large enough V will be positive,

$$V = \int_0^1 \frac{x^2(1-x^2)(1-p^2x^2)dx}{\{1+(2p+\tau^2)x^2+p^2x^4\}^{\frac{3}{2}}};$$

thus the sign of V is the same as that of

$$\int_0^1 \frac{x^2(1-x^2)(1-p^2x^2)dx}{(c^2+x^2+p^2c^2x^4)^{\frac{3}{2}}},$$

where c^2 stands for $\frac{1}{2p+\tau^2}$.

When c is made small enough, the term $p^2c^2x^4$ may be neglected in comparison with x^2 ; and so the sign of the integral will become the same as that of

$$\int_0^1 \frac{x^2(1-x^2)dx}{(c^2+x^2)^{\frac{3}{2}}} - \int_0^1 \frac{p^2x^4(1-x^2)dx}{(c^2+x^2)^{\frac{3}{2}}}.$$

The first term is infinite when $c=0$, and the second term is finite; thus the sign of the integral is positive when c is small enough. Since V is negative when $\tau=0$, and is positive when τ is large enough, it must vanish for some intermediate value of τ .

15. Moreover, when c is very small, we have approximately

$$\int_0^1 \frac{x^2(1-x^2)dx}{(c^2+x^2)^{\frac{3}{2}}} = \log \frac{2}{c} - \frac{3}{2} = \log \frac{2}{ce\sqrt{e}}.$$

$$\int_0^1 \frac{p^2x^4(1-x^2)dx}{(c^2+x^2)^{\frac{3}{2}}} = \frac{p^2}{4}.$$

Thus to make V vanish when τ is *very large*, we have approximately

$$\frac{2}{ce\sqrt{e}} = e^{\frac{p^2}{4}},$$

therefore

$$2(2p+\tau^2)^{\frac{1}{2}} = e^{\frac{p^2}{4} + \frac{3}{2}};$$

so that approximately

$$\tau = \frac{1}{2}e^{\frac{p^2}{4} + \frac{3}{2}}.$$

16. We shall next show that corresponding to a given value of p there is *only one value* of τ which will make V vanish.

Put

$$\Delta^2 = (1+px^2)^2 + \tau^2x^2,$$

$$P^2 = (1+p)^2 + \tau^2;$$

then

$$(1-x^2)(1-p^2x^2) = \Delta^2 - P^2x^2.$$

Thus we have

$$V = \int_0^1 \frac{x^2dx}{\Delta} - P^2 \int_0^1 \frac{x^4dx}{\Delta^3};$$

and therefore

$$\frac{dV}{rdr} = -3 \int_0^1 \frac{x^4 dx}{\Delta^3} + 3P^2 \int_0^1 \frac{x^2 dx}{\Delta^3}.$$

Hence

$$V + \frac{P^2}{3} \frac{dV}{rdr} = \int_0^1 \frac{x^2 dx}{\Delta} \left(1 - \frac{P^2 x^2}{\Delta^2}\right)^2 dx. \quad \dots \quad (9)$$

The right-hand member is necessarily positive. If we put $\sigma = r^2$, we may write the equation thus,

$$\frac{d}{d\sigma}(V \cdot P^2) = \frac{3}{2} P \int_0^1 \frac{x^2 dx}{\Delta} \left(1 - \frac{P^2 x^2}{\Delta^2}\right)^2 dx.$$

This shows that if p be kept constant, VP^2 continually increases with σ , and therefore cannot vanish more than once, and therefore of course V cannot vanish more than once.

17. Ivory makes the following statement:—

“Let V stand for the integral in the equation (7); and supposing that p and r^2 vary so as always to satisfy that equation, we shall have

$$\frac{dV}{dp} dp + \frac{dV}{rdr} r dr = 0.$$

Now, r^2 representing any positive quantity, we may conceive it to increase from zero to be infinitely great; in which case it follows from the nature of the function V , that during the whole increase $\frac{dV}{rdr} r dr$ will be negative: wherefore the other term $\frac{dV}{dp} dp$ will be positive; which requires that p decrease continually.”

It is here in fact asserted that $\frac{dV}{dr}$ is negative for such values of p and r as make V vanish; this is, however, wrong, as equation (9) shows that $\frac{dV}{dr}$ is positive when $V=0$. The mistake was pointed out by Liouville in the memoir already cited, and the correction was accepted by Ivory in the *Philosophical Transactions* for 1839, pages 265, 266. The mistake vitiates the remainder of Ivory's memoir.

The investigation of Art. 16 is taken in substance from Liouville's memoir; he also demonstrates the proposition of Art. 14, but not in the way which I have adopted.

18. The extract given in the preceding article from Ivory's memoir involves another error, which Liouville does not notice: the words “which requires that p decrease continually,” contain an arbitrary unproved assertion. We have

$$\frac{dV}{dp} \frac{dp}{dr} + \frac{dV}{dr} = 0;$$

hence if $\frac{dV}{dr}$ were negative, $\frac{dV}{dp} \frac{dp}{dr}$ would be positive; then $\frac{dp}{dr}$ might be

positive or it might be negative: we cannot assert at once, as Ivory does, that $\frac{dp}{d\tau}$ must be negative. Suppose, for example, that V stood for $p^2 - \tau^2 - 1$; then $\frac{dV}{d\tau}$ would be negative for positive values of τ , and p would continually increase with τ .

19. As I have already stated, Ivory accepted the correction made by Liouville; but in the two pages in which the mistake is acknowledged other untenable assertions are advanced. It was in effect necessary for Ivory's purpose to trace the curve determined by $V=0$ in the first quadrant; but instead of *demonstration* such as we have supplied in Art. 14, Ivory gives unwarranted assertions of the kind already noticed in Art. 18.

We have

$$\frac{dV}{dp} = - \int_0^1 \frac{x^4(1-x^2)}{\Delta^3} \{ (3+2p-p^2x^2)(1+px^2) + 2pr^2x^2 \} dx,$$

from which it follows that, whatever positive number τ^2 stands for, $\frac{dV}{dp}$ is negative for all values of p that make $3+2p-p^2$ positive, that is, for all values of p less than 3. This Ivory gives, and so far he is correct; $\frac{dV}{dp}$ is certainly negative, and not zero, so long as p is less than 3. Ivory wishes to show that $\frac{dV}{dp}$ can never be zero. He takes the differential equation

$$\frac{dV}{dp} dp + \frac{dV}{d\tau} \tau d\tau = 0;$$

he says, "Further, in the differential equation $\frac{dV}{dp}$ cannot be zero; because, τ^2 increasing without limit, $\frac{dV}{d\tau} \tau d\tau$ is essentially positive."

This is quite unsound. Regard p as the abscissa and τ as the corresponding ordinate of a curve determined by $V=0$; then assuming that $\frac{dV}{d\tau}$ is always positive, yet $\frac{dV}{dp}$ may vanish; that is, there may be a point or points on the curve at which the tangent is parallel to the axis of abscissæ. Suppose, for example, that V stood for

$$\tau^3 - p^3 + 4ap^2 - 5a^2p - b^3;$$

then $\frac{dV}{d\tau}$ is always positive, $\frac{dV}{dp}$ is negative when p is less than a , and vanishes when $p=a$ or $\frac{5a}{3}$.

I do not assert that $\frac{dV}{dp}$ can vanish in the present case; I only maintain that Ivory's argument to show that $\frac{dV}{dp}$ cannot vanish is unsound. I shall

presently demonstrate that $\frac{dV}{dp}$ cannot vanish.

20. Ivory concludes the two pages in the Philosophical Transactions for 1839 thus:—

“If the sign of $\frac{dV}{dp}$ be changed, the result will be positive; and hence, observing that x is contained between 0 and 1, we obtain a condition between any two values of p and r^2 that satisfy the equation (7), namely, the expression

$$(3+p-p^2)(1+p)+2pr^2$$

must be a positive quantity, or, which is the same thing,

$$r^2 > (p^2 - p + 3) \frac{1+p}{2p}.”$$

This is obscure in its commencement; but the statement to which it proceeds is intelligible, but is unwarranted. For, granting that $\frac{dV}{dp}$ is always to be negative, this will be secured if

$$(3+2p-p^2x^2)(1+px^2)+2pr^2x^2$$

is always positive; that is, if

$$3+2p+(3p+p^2+2pr^2)x^2-p^2x^4$$

is always positive; that is, if

$$\frac{3+2p}{x^2}+3p+p^2+2pr^2-p^2x^2$$

is always positive. And as x lies between 0 and 1, this condition is certainly secured if

$$3+2p+3p+p^2+2pr^2 \text{ is greater than } p^2.$$

According to Ivory's statement it would be *necessary* that

$$3+4p+2pr^2 \text{ should be greater than } p^2;$$

whereas we see that it would be *sufficient* that

$$3+5p+p^2+2pr^2 \text{ should be greater than } p^2.$$

Ivory's second statement is inconsistent with his first, but becomes consistent with it if we change $+3$ to -3 ; if, however, his second statement is to be taken as what he intended, and his first statement corrected to agree with it, his error is aggravated.

In fact, however, I doubt whether any such *necessary* criterion as Ivory proposes can be easily deduced from the value of $\frac{dV}{dp}$. For, granting that $\frac{dV}{dp}$ is always negative and never zero, it will not follow that *every element* in the integral which expresses $\frac{dV}{dp}$ must be negative, but only that the

aggregate of the positive elements, if such there be, should fall short of the aggregate of the negative elements. However, be this as it may, there can be no doubt that the specific criterion which Ivory proposes is quite unsupported by demonstration.

When p and r are very large, the relation between them is approximately that given in Art. 15.

21. In Arts. 14 and 16 it is shown that for every given value of p greater than p_0 , there is one, and only one, value of r which will make V vanish. We shall now show that corresponding to every value of r there is one, and only one, value of p which will make V vanish.

Whatever be the given value of r , it is obvious that V is positive when $p=0$, and negative when p is large enough: thus there must be some intermediate value of p which makes V vanish.

We have then to show that there is only *one* such value; to show this we shall demonstrate the proposition which Ivory asserted on insufficient grounds, namely, that $\frac{dV}{dp}$ can never vanish simultaneously with V .

$$\frac{dV}{dp} = - \int_0^1 \frac{x^4(1-x^2)}{\Delta^4} \left\{ 3 + 2p + (3p + p^2 + 2pr^2)x^2 - p^2x^4 \right\} dx;$$

and we know that $\frac{dV}{dp}$ cannot vanish so long as p is less than 3. See Art. 19.

We shall first show that the limit $p=3$ may be changed to a larger limit.

If the integral

$$\int_0^1 x^4(1-x^2) \{ 3 + 2p + (3p + p^2 + 2pr^2)x^2 - p^2x^4 \} dx$$

is positive, the integral will also be positive when Δ^4 is introduced as a divisor under the integral sign; for by introducing this denominator we diminish all the elements of the integral, but we diminish every negative element in a higher degree than any positive element.

Now the value of the last integral is

$$\frac{2(3+2p)}{5 \times 7} + \frac{2(3p+p^2+2pr^2)}{7 \times 9} - \frac{2p^2}{9 \times 11};$$

we are certain that this is positive if

$$\frac{2pr^2+p^2+3p}{7 \times 9} + \frac{3+2p}{5 \times 7} \text{ is not less than } \frac{p^2}{9 \times 11},$$

that is, if

$$r^2 + \frac{3+p}{2} + \frac{9(3+2p)}{10p} \text{ is not less than } \frac{7p^2}{22}.$$

On trial it will be found that this condition is satisfied, even when r is zero, provided p be not greater than $4\frac{1}{2}$. Thus we have only to consider

the case in which both p is greater than $4\frac{1}{2}$, and $r^2 + \frac{3+p}{2} + \frac{9(3+2p)}{10p}$ is less than $\frac{7p^2}{22}$; and we have to show that $\frac{dV}{dp}$ is negative in this case.

Put $p = \frac{1}{q}$; thus V becomes a function of q and r ; and we have to show that $\frac{dV}{dq}$ cannot vanish when q lies between 0 and $\frac{3}{13}$, and $r^2 + \frac{3q+1}{2q} + \frac{9(3q+2)}{10}$ is less than $\frac{7}{22q^2}$, so that $r^2q^2 + \frac{(3q+1)q}{2} + \frac{9(3q+2)q^2}{10}$ is less than $\frac{7}{22}$. We have

$$V = q \int_0^1 \frac{x^2(1-x^2)(q^2-x^2)dx}{D^3},$$

where D^2 stands for $q^2 + 2qx^2 + x^4 + q^2r^2x^2$; therefore

$$\frac{dV}{dq} = \frac{V}{q} + q \int_0^1 \frac{x^2(1-x^2)}{D^3} \left[2q - \frac{3(q^2-x^2)\{q+(1+qr^2)x^2\}}{D^2} \right] dx.$$

Separate the integral into two parts, one extending between the limits $x=0$ and $x=q$, and the other between the limits $x=q$ and $x=1$. It is obvious that the second part is positive, so that we have only the first part to examine; this is

$$\int_0^q \frac{x^2(1-x^2)}{D^3} \left\{ -q^3 + (3q+q^2-r^2q^2)x^2 + (3+2q+3qr^2)x^4 \right\} dx,$$

and we shall show that this is positive.

Let Q stand for

$$-q^3 + (3q+q^2-r^2q^2)x^2 + (3+2q+3qr^2)x^4;$$

then we shall first show that

$$\int_0^q (1-x^2)Qdx$$

is positive; and next that

$$\int_0^q \frac{x^2(1-x^2)Qdx}{D^3}$$

is positive.

Now $\int_0^q (1-x^2)Qdx$ will certainly be positive if

$$\int_0^q (1-x^2) \left\{ -q^3 + (3q+q^2-r^2q^2)x^2 + 3qr^2x^4 \right\} dx$$

is positive. The last integral is

$$-q^3\left(q-\frac{q^3}{3}\right) + \left(3q+q^2-r^2q^2\right)\left(\frac{q^3}{3}-\frac{q^5}{5}\right) + 3qr^2\left(\frac{q^5}{5}-\frac{q^7}{7}\right).$$

that is,

$$\frac{q^5}{3} \left(1 - \frac{4q}{5} - \frac{3q^2}{5} \right) + \frac{4q^5 r^2}{15} \left(1 - \frac{6q^2}{7} \right);$$

and this is positive, for q does not exceed $\frac{3}{13}$.

Now the coefficient of x^2 in Q is positive, since $r^2 q^3$ is less than $\frac{q}{3}$; and thus Q cannot change sign more than once; it changes sign once, for it is negative when $x=0$, and positive when $x=q$. The factor $\frac{x^2}{D^5}$ will be found to *increase continually* as x increases from 0 to q . Hence in passing from $\int_0^q (1-x^2)Qdx$ to $\int_0^q \frac{x^2(1-x^2)Qdx}{D^5}$, a larger fraction is taken of every positive element than of any negative element; so that since the former integral has been shown to be positive, the latter is necessarily positive.

To demonstrate that $\frac{x^2}{D^5}$ continually increases as x increases from 0 to q , take the differential coefficient with respect to x ; the sign of this differential coefficient is the same as that of

$$2xD^2 - 5x^2(2qx + q^2 r^2 x + 2x^3),$$

that is, of

$$2D^2 - 5x^2(2q + q^2 r^2 + 2x^2),$$

that is, of

$$2q^2 - (6q + 3q^2 r^2)x^2 - 8x^4;$$

this is positive when $x=0$; when $x=q$ it becomes $q^2(2-6q-3q^2 r^2-8q^2)$, which we shall now show to be positive. We have by supposition

$$3r^2 q^2 + \frac{3q}{2} + \frac{99q^2}{10} + \frac{81q^3}{10} \text{ less than } \frac{21}{22},$$

and

$$2-6q-3q^2 r^2-8q^2 = 2 - \frac{9q}{2} - \left(\frac{3q}{2} + 3q^2 r^2 + 8q^2 \right);$$

and as q does not exceed $\frac{3}{13}$, this is greater than $2 - \frac{27}{26} - \frac{21}{22}$, and so is

positive. Hence we see that when $V=0$, the value of $\frac{dV}{dq}$ is necessarily positive, and cannot be zero.

22. We have shown in the preceding Article, that $\frac{dV}{dp}$ cannot vanish simultaneously with V ; the demonstration is rather complex, and perhaps for this reason Ivory attempted to establish the proposition by unsound general reasoning. Liouville does not give the proposition, although it is naturally required to make the discussion of the admissible values of p and r complete.

23. Thus we may state the results of Arts. 14, 16, and 21 in the fol-

lowing manner:—If the curve determined by the equation $V=0$ be traced in the first quadrant, every straight line parallel to the axis of p meets the curve once, and only once, and every straight line parallel to the axis of r , and at a greater distance from it than p_0 , meets the curve once, and only once. As $\frac{dV}{dr}$ vanishes with r , the curve meets the axis of p at right angles ;

and from Art. 15 it follows that when r and p are indefinitely great, the angle which the tangent to the curve makes with the axis of p is very nearly a right angle. Thus, for small values of r the curve is concave to the axis of p , and for very large values of r the curve is convex to the axis of p ; so that the curve must have a point or points of inflexion.

24. In the very careful account of Ivory's mathematical researches, which is given in the fourth volume of the 'Abstracts of the Papers . . . of the Royal Society,' it is said, with respect to Jacobi's theorem, "In a paper in the Transactions for 1838, Mr. Ivory has with great elegance demonstrated this theorem, and has given, with greater detail than its authors had entered on, several statements regarding the limitations of the proportions of the axes." The language is cautious, but seems to imply some suspicion with regard to the accuracy of the statements. As we have now seen, many of Ivory's statements are inaccurate, and others, though accurate, are based on unsound reasoning.

"On the Theory of Continuous Beams." By JOHN MORTIMER HEPPEL, M. Inst. C.E. Communicated by Prof. W. J. MACQUORN RANKINE. Received December 9, 1869*.

In venturing to present to the Royal Society a paper on a subject which has engaged the attention, more especially in France, of some of the most eminent engineers and writers on Mechanical Philosophy, the author feels it to be incumbent on him to state the nature of the claim to their attention which he hopes it may be found to possess in point of originality or improvement on the method of treatment.

To do this clearly, however, it will be necessary to advert to the principal steps by which progress in the knowledge of this subject has been made, both in France and in this country.

The theory of continuous beams appears to have first attracted attention in France about 1825, when a method of determining all the conditions of equilibrium of a straight beam of uniform section throughout, resting on any number of level supports at any distances apart, each span being loaded uniformly, but the uniform loads varying in any manner from one span to another, was investigated and published by M. Navier. This method, although perfectly exact for the assumed conditions, was objectionable from the great labour and intricacy of the calculations it entailed.

* Read January 27, 1870. See vol. xviii. p. 176.

Messrs. Molinos and Pronnier, in their work entitled 'Traité Théorique et Pratique de la construction des Ponts Métalliques,' describe this process fully, and show that for a bridge of n openings, the solution must be effected of $3n + 1$ equations, involving as many unknown quantities, these equations being themselves of a complex character; and they observe, "Thus to find the curve of the moments of rupture for a bridge of 6 spans 19 equations must be operated on; such calculations would be repulsive, and when the number of spans is at all considerable this method must be abandoned."

The method of M. Navier, however, remained the only one available till about 1849, when M. Clapeyron, Ingénieur des Mines, and Member of the Academy of Sciences, being charged with the construction of the Pont d'Asnières, a bridge of five continuous spans over the Seine, near Paris, applied himself to seek some more manageable process. He appears to have perceived (and so far as the writer is informed, to have been the first to perceive) that if the bending moments over the supports at the ends of any span were known as well as the amount and distribution of the load, the entire mechanical condition of this portion of the beam would become known just as if it were an independent beam. Upon this M. Clapeyron proceeded to form a set of equations involving as unknown quantities the bending moments over the supports, with a view to their determination. He found himself, however, obliged to introduce into these equations a second set of unknown quantities ("*inconnues auxiliaires*"), being the inclinations of the deflection curve at the points of support, and not having arrived at a general method of eliminating these latter, was obliged to operate in each case on a number of equations equal to twice the number of spans. M. Clapeyron does not appear, as yet, to have made any formal publication of his method, but to have used it in his own practice, and communicated it freely to those with whom he came in contact.

In 1856, M. Bertot, Ingénieur Civil, appears to have found the means of eliminating this second set of unknown quantities $n + 1$ in number for a bridge of n spans, and thus reducing the number of equations to $n - 1$.

Each of these equations involved as unknown quantities the bending moments over three consecutive supports, and was considered, from its remarkable symmetry and simplicity, to merit a distinctive name, that of "The Theorem of the three Moments."

The method, however, to which this theorem is the key, is still everywhere called that of M. Clapeyron, and, as it appears to the writer, justly so, as it was an immediate and simple result from his investigations, with which M. Bertot was well acquainted.

The next important advance was made in 1861, when M. Bresse, Professeur de Mécanique appliquée à l'École Impériale des Ponts et Chaussées, completed the matter of the third volume of his course, which is exclusively devoted to this subject*. M. Bresse explains and de-

* This was communicated to the Academy of Sciences in 1862, though the volume was not published till 1865.

monstrates the theorem of the three moments, at the knowledge of which he had himself arrived from M. Clapeyron's investigations, independently of M. Bertot. He then goes on to the investigation of an equation of much greater generality, in which what is termed by English writers "imperfect continuity" is taken into account, being, however, there replaced by the precisely equivalent notion of original differences of level in the supports, the beam being always supposed primitively straight; besides this the loads, instead of being taken as uniform for each span, are considered as distributed in any given manner.

Having obtained this fundamental equation, M. Bresse proceeds to investigate the nature of the curves, which are the envelopes of the greatest bending moments produced at each point, by the most unfavourable distribution of the load in reference to it, and finally gives tables for the ready calculation of results in a great variety of cases, comprising most of those likely to occur in practice.

During the time that M. Bresse was engaged in these researches, an Imperial Commission was formed, of which he was a member, for the purpose of devising rules applicable to practice, and the results of his labours have been the basis of legislative enactments equivalent to our Board of Trade regulations prescribing the methods to be followed in determining the stresses in the various parts of the structure.

About the same time that M. Bresse turned his attention to this subject, it appears also to have engaged that of M. Bélanger, who in his work entitled '*Théorie de la Résistance et de la Flexion Plane des Solides &c.*, Paris, 1862,' gives a very complete demonstration—resulting in an equation which in one point of view is slightly more general than that of M. Bresse, as it takes in variation of the moment of inertia of the section from one span to another. In another point of view its generality is slightly less, as it deals only with loads distributed over each separate span uniformly, whereas M. Bresse replaces the simple algebraical terms expressing these by definite integrals expressing the load as a function of the distance from one of the points of support.

As far as the writer is informed, little has been done in France to advance this theory beyond the point to which it was brought by the writers last mentioned, and especially by M. Bresse; but valuable contributions to its development in reference to application to practice are to be found in the work of MM. Molinos and Pronnier above referred to, as well as in various papers by MM. Renaudot, Albaret, Colignon, Piarron de Mondesir, &c.

In England little or no attention appears to have been paid to this subject by writers on mechanics till 1843, when the Rev. Henry Moseley, Professor of Natural Philosophy and Astronomy at King's College, London, published his work on '*The Mechanical Principles of Engineering and Architecture.*' In part 5 of this work, which treats of the strength of materials, four cases of continuous beams are fully investigated, and the

general case is to a certain extent discussed, the method of M. Navier being perhaps rather indicated than fully developed.

Prof. Moseley's work was altogether a most valuable contribution to engineering science, and, as far as the present subject is concerned, no doubt furnished the groundwork of the method applied by Mr. Pole to the solution of other particular but more complex and difficult cases.

The first case which engaged the attention of Mr. Pole appears to have been that of the bridge over the Trent at Torksey, consisting of two spans of continuous tubular beams, resting on abutments and a central pier. For special reasons it had become necessary that the real conditions of equilibrium of this bridge should be investigated with more than ordinary precision; and this Mr. Pole did by a method virtually identical with that of M. Navier, though it does not appear that he had any previous knowledge of that method, except through the medium of Moseley's work. Throughout Moseley's cases, however, the load on the beam is considered as distributed uniformly over its entire length, whereas Mr. Pole had to deal with the case of different loads on the two spans, and no doubt had to devise the method of analysis necessary for its treatment. Mr. Pole's paper on this subject is published in vol. ix. of the 'Minutes of Proceedings Inst. Civ. En.' 1849-50.

As far as this went, however, it could hardly be considered to have advanced the theory of the subject, as M. Navier's method included this case, and much more; but about the same time Mr. Pole had to investigate the case of a much larger work, the Britannia Bridge, where he had to deal with some new conditions, which, as far as the writer is aware, were then for the first time successfully treated.

These were that, besides variation of load on the different spans, their cross sections also varied, and there was imperfect continuity over the centre pier, that is to say, that the points of support being supposed to range in a straight line, the beam if relieved from all weight would cease to remain in contact with them all, and would consist of two equal straight portions, forming an angle pointing upwards. The process, which for distinction may be called that of M. Navier, was skilfully extended by Mr. Pole so as to include these new circumstances, and by its means results were obtained certainly true within a very small limit, and as near the absolute truth as any existing means of treating the subject would produce.

Mr. Pole's researches on this subject are published in Mr. Edwin Clark's work on the Britannia and Conway Bridges, 1850. Both from the clear and accurate treatment of the case and the record of the numerous and delicate observations by which the theoretical conclusions were continually verified and kept in check, they are most strongly to be recommended to the attention of engineers having to deal with works of this character.

The sequence of events now compels the writer to advert to some studies of his own. In 1858-59, being then Chief Engineer of the Madras Rail-

way, he had occasion to investigate the conditions of a bridge of five continuous spans over the River Palar. Having in India no books to refer to but those of Moseley and Edwin Clark, he found himself unable to extend the treatment of the cases there given to that of a beam with an increased number of openings and varying loads. After many attempts and failures, the same idea occurred to him which appears to have struck M. Clapeyron nine or ten years before, that if the bending moments over the supports were known, the whole conditions would become known.

Following this clue, he was fortunate enough to succeed in at once eliminating the other unknown quantities, which M. Clapeyron had been obliged to retain in his equations for many years after his original discovery of the method, and thus to arrive at an equation precisely identical with that which had been first published in France by M. Bertot in 1856, and was known as the "Theorem of the three Moments."

This was sufficient for the immediate purpose, as the beams in question were straight and of uniform section throughout, conditions to which this theorem is strictly applicable without any modification whatever.

As, however, the writer was at this time under the impression that he was using an entirely new mode of analysis, he was naturally anxious to check its results by comparison with those obtained in some well-known case by other means. Fortunately he had at hand that of the Britannia Bridge, perhaps the best that could have been selected; but for this purpose it became necessary to import into the fundamental equation the conditions of varying sections in the different spans and imperfect continuity. This, however, presented no great difficulty, and by means of an equation thus modified, he had the satisfaction of reproducing all Mr. Pole's results, and thus convincing himself of the trustworthiness of the method in question.

The equation thus generalized is absolutely identical with that arrived at by M. Bélanger in the work above referred to*.

It would appear, then, that the theory of this subject was independently advanced to about the same state of perfection in France and in England, though as regards the development of its application to practice no doubt very much the more has been done in the former country.

The writer will now advert to some inherent defects of this theory, the cure of which is the principal object of the investigation which follows.

The chief one, which is admitted by all writers on the subject, is the necessity for supposing the moment of inertia of the section constant throughout each span; any more general hypothesis, it is said, would render the calculation inextricable. Still it is certain that the conclusions arrived at on the hypothesis of a constant section cease to be true if a variation of section is introduced, and the amount of error thereby induced, though considered to be probably small, is still a matter of uncertainty.

The next defect is the assumption of uniformity of load throughout

* A paper on this subject by the writer was published in the Minutes of Proceedings Inst. C.E. vol. xix. 1859-60.

each span ; for although as far as rolling load is concerned no more correct hypothesis could be made, the weight of the bridge itself, if a large one, usually varies considerably in the different parts of the same span.

The equation given by M. Bresse, as has been stated, provides for certain kinds of variable loads by the use of integrals, but the writer is not aware that they have been applied, even by that author himself, to the purposes of calculation, and it seems to him that in most cases the attempt to make such an application would be beset with difficulties.

It will, however, it is hoped, be seen from what follows, that the dealing with variations of the above elements does not in fact present any very formidable difficulty, though no doubt the labour of calculation is greater ; but what the writer regards as most satisfactory is the very small difference in the principal results in the case of the Britannia (where these variations greatly exceed in amount those usually occurring), whether obtained by the approximate method hitherto followed, or by the more rigorous one to be explained, affording a strong presumption that in all ordinary cases the former method may be confidently employed without risk of any important error.

Should the following treatment of the case be deemed successful, the author would remark that its success is mainly due to the use of an abbreviated functional notation, by which a great degree of clearness and symmetry is preserved in expressions which would otherwise have become inextricably complex.

General Investigation of the Bending Moments and Deflections of Continuous Beams.



Let 1 . 2 represent any span of a continuous beam, the length of the span being l .

x, y the coordinates of the deflection curve, the origin being at the point 1.

a and b particular values of x .

e_1, e_2, e_3 reciprocals of the products of the moments of inertia of the sections in the spaces (1 . a), (a . b), (b . 2), about their neutral axes, by the modulus of elasticity of the material $\left(\frac{1}{EI}\right)$.

μ_1, μ_2, μ_3 loads per unit of length in the same spaces.

T tangent of inclination of deflection curve at 1, to straight line joining 1 . 2, its positive value being taken upwards.

ϕ_1, ϕ_2 bending moments at 1 . 2.

P shearing force at 1.

Now let the bending moment at any point (x, y)

between 1 and a be called $F_1''(x)$,

between a and b be called $F_2''(x)$,

between b and 2 be called $F_3''(x)$;

and let the part of this bending moment, which results alone from the load on the beam between l and x , be called

between 1 and $a f_1''(x)$,
between a and $b f_2''(x)$,
between b and $2 f_3''(x)$;

and let the first and second integrals of these functions, as of $F_1''(x)$, $f_1''(x)$, be denoted by $F_1'(x)$, $f_1'(x)$, and $F_1(x)$, $f_1(x)$, and the value of any one, as $F_1(x)$, for a particular value of x , as a by $F_1(a)$;

then $f_1''(x) = \mu_1 \frac{x^2}{2}, \dots \dots \dots (1)$

$$f_2''(x) = \mu_1 a \left(x - \frac{a}{2} \right) + \mu_2 \frac{(x-a)^2}{2}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (2)$$

$$f_3''(x) = \mu_1 a \left(x - \frac{a}{2} \right) + \mu_2 (b-a) \left(x - \frac{b+a}{2} \right) + \mu_3 \frac{(x-b)^2}{2}. \quad (3)$$

Also, from equality of moments about the point (x, y) ,

$$\mathbf{F}_i''(x) = \phi_i - \mathbf{P}x + \mathbf{f}_i''(x), \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

$$\mathbf{F}_2''(x) = \phi_1 - Px + f_2''(x), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

$$\mathbf{F}_3''(x) = \phi_1 - \mathbf{P}x + f_3''(x); \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

and, from equality of moments about the point 2,

$$Pl = \phi_1 - \phi_2 + f_3''(l),$$

$$P = \frac{1}{l} (\phi_1 - \phi_2 + f_s''(l)). \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Substituting for P in (4), (5) and (6),

$$\mathbf{F}_1''(x) = \left(1 - \frac{x}{l}\right) \phi_1 + \frac{x}{l} \phi_2 - \frac{x}{l} f_3''(l) + f_1''(x), \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

$$\mathbf{F}_2''(x) = \left(1 - \frac{x}{l}\right) \phi_1 + \frac{x}{l} \phi_2 - \frac{x}{l} f_3''(l) + f_2''(x), \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

$$F_3''(x) = \left(1 - \frac{x}{l}\right)\phi_1 + \frac{x}{l}\phi_2 - \frac{x}{l}f_3''(l) + f_3''(x), \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

equations from which for a given value of x , $F_1''(x)$, $F_2''(x)$, $F_3''(x)$ may be determined if ϕ_1 and ϕ_2 are known.

From the nature of the deflection curve,

from 1 to a ,

[illegible]

from a to b ,

[illegible]

there were any number of particular values of x to be considered, as a, b &c., j, k, l , the corresponding values of $\frac{1}{EI}$ being e_1, e_2 &c., e_{n-1}, e_n , it might be written

$$y = \left[\begin{aligned} &e_1(F_1(a) + F_1'(a)(x-a)) \\ &+ e_2[(F_2(b) + F_2'(b)(x-b)) - (F_2(a) + F_2'(a)(x-a))] \\ &+ e_3[(F_3(c) + F_3'(c)(x-c)) - (F_2(b) + F_2'(b)(x-b))] \\ &+ \&c. \\ &+ e_{n-1}[(F_{n-1}(k) + F_{n-1}'(k)(x-k)) - (F_{n-1}(j) + F_{n-1}'(j)(x-j))] \\ &+ e_n[F_n(x) - (F_n(k) + F_n'(k)(x-k))] \end{aligned} \right] - Tx; \quad (24)$$

[$x=l$ in (24), $y=0$;

$$T = \frac{1}{l} \left[\begin{aligned} &e_1[F_1(a) + F_1'(a)(l-a)] \\ &+ e_2[(F_2(b) + F_2'(b)(l-b)) - (F_2(a) + F_2'(a)(l-a))] \\ &+ e_3[(F_3(c) + F_3'(c)(l-c)) - (F_2(b) + F_2'(b)(l-b))] \\ &+ \&c. \\ &+ e_{n-1}[(F_{n-1}(k) + F_{n-1}'(k)(l-k)) - (F_{n-1}(j) + F_{n-1}'(j)(l-j))] \\ &+ e_n[F_n(l) - (F_n(k) + F_n'(k)(l-k))] \end{aligned} \right] \quad \dots \quad (25)$$

If, now, the formation of the functions $F_1(a), F_1'(a)$ &c. be examined, it is evident that this equation may be written

$$T = A\phi_1 + B\phi_2 + C,$$

where A and B are known functions of a, b, c , &c., and e_1, e_2, e_3 , &c., and C is a known function of the same, and μ_1, μ_2, μ_3 , &c.

If the adjacent span to the left be now considered, it is evident that a precisely similar equation may be obtained, which may be written

$$T' = A'\phi_1 + B'\phi_2 + C';$$

adding these, and writing t for $T + T'$, which is known as it is the tangent of the small angle which the neutral lines of the two spans would make at the point 1 if relieved from all load,

$$t = (A + A')\phi_1 + B\phi_2 + B'\phi_2 + C + C',$$

which may be written

$$\Psi_1(\phi_0, \phi_1, \phi_2) = 0;$$

similarly for the other bearing points in succession,

$$\Psi_2(\phi_1, \phi_2, \phi_3) = 0,$$

$$\Psi_3(\phi_2, \phi_3, \phi_4) = 0, \&c.,$$

where the number of equations is two less than that of the quantities

ϕ_0, ϕ_1 , &c., so that if two of these are known the rest may be determined. But the first and last are always known, being usually each $=0$. Therefore they may all be determined.

This being so, the bending moment at any point (x, y) may be found from equations (8), (9), (10) and others of the same form; and the deflection may be found from equations (19), (21), (23), and others of the same form, regard being had to the interval of the beam in which the point under examination lies.

If, now, we suppose that $a=b=c=\&c.=l$, equation (25) reduces to

$$T = \frac{1}{EI} (F_1(l));$$

similarly,

$$T' = \frac{1}{EI'} (F_1(l'));$$

$$\therefore t = \frac{1}{EI} F_1(l) + \frac{1}{EI'} F_1(l'),$$

$$EI t = F_1(l) + i(F_1(l')), \text{ writing } i \text{ for } \frac{1}{I'},$$

$$= \left(\frac{l}{3} + \frac{il'}{3}\right) \phi_1 + \frac{l}{6} \phi_2 + \frac{il}{6} \phi_0 - \frac{l^3}{24} \mu - \frac{il'^3}{24} \mu'.$$

Clearing of fractions and transposing,

$$8(l + il') \phi_1 + 4l \phi_2 + 4il' \phi_0 = l^3 \mu + il'^3 \mu' + 24EI t, \quad (26)$$

an equation which was given by the author in his paper before referred to, and which is nearly identical with the general equation of M. Bresse, and allowing for difference of notation precisely so with that of M. Bélanger.

If $i=1$ and $t=0$, which is the case of a straight beam of uniform section throughout,

$$8(l + l') \phi_1 + 4l \phi_2 + 4l' \phi_0 = l^3 \mu + l'^3 \mu', \quad (27)$$

which is the equation generally known as the theorem of the three moments.

If in equation (25) we put $l=a$, it becomes

$$T = a e_1 \left(\frac{1}{3} \phi_1 + \frac{1}{6} \phi_2 - \frac{1}{24} a^2 \mu_1 \right); \quad (28)$$

and for the central deflection equation (19) becomes

$$Y = a^2 e_1 \left(-\frac{1}{16} (\phi_1 + \phi_2) + \frac{5}{384} a^2 \mu_1 \right). \quad (29)$$

If we put $b=2a, l=3a$,

$$T = a \left[\begin{aligned} &e_1 \left(\frac{19}{27} \phi_1 + \frac{7}{54} \phi_2 - a^2 \left(\frac{43}{216} \mu_1 + \frac{7}{36} \mu_2 + \frac{7}{108} \mu_3 \right) \right) \\ &+ e_2 \left(\frac{7}{27} \phi_1 + \frac{13}{54} \phi_2 - a^2 \left(\frac{7}{54} \mu_1 + \frac{7}{24} \mu_2 + \frac{13}{108} \mu_3 \right) \right) \\ &+ e_3 \left(\frac{1}{27} \phi_1 + \frac{7}{54} \phi_2 - a^2 \left(\frac{1}{54} \mu_1 + \frac{1}{18} \mu_2 + \frac{11}{216} \mu_3 \right) \right) \end{aligned} \right], \quad (30)$$

and central deflection from equation (21),

$$Y=a^2 \left[\begin{aligned} &e_1 \left(-\frac{7}{36}\phi_1 - \frac{1}{18}\phi_2 + a^2 \left(\frac{11}{144}\mu_1 + \frac{1}{12}\mu_2 + \frac{1}{36}\mu_3 \right) \right) \\ &+ e_2 \left(-\frac{5}{16}\phi_1 - \frac{5}{16}\phi_2 + a^2 \left(\frac{5}{32}\mu_1 + \frac{47}{128}\mu_2 + \frac{5}{32}\mu_3 \right) \right) \\ &+ e_3 \left(-\frac{1}{18}\phi_1 - \frac{7}{36}\phi_2 + a^2 \left(\frac{1}{36}\mu_1 + \frac{1}{12}\mu_2 + \frac{11}{144}\mu_3 \right) \right) \end{aligned} \right] \dots \dots \dots (3)$$

If we put $b=2a$, $c=3a$, $d=4a$, $l=5a$,

$$Y=a \left[\begin{aligned} &e_1 \left(\frac{61}{75}\phi_1 + \frac{13}{150}\phi_2 - a^2 \left(\frac{149}{600}\mu_1 + \frac{91}{300}\mu_2 + \frac{13}{60}\mu_3 + \frac{13}{100}\mu_4 + \frac{13}{300}\mu_5 \right) \right) \\ &+ e_2 \left(\frac{37}{75}\phi_1 + \frac{31}{150}\phi_2 - a^2 \left(\frac{37}{150}\mu_1 + \frac{123}{200}\mu_2 + \frac{31}{60}\mu_3 + \frac{31}{100}\mu_4 + \frac{31}{300}\mu_5 \right) \right) \\ &+ e_3 \left(\frac{19}{75}\phi_1 + \frac{37}{150}\phi_2 - a^2 \left(\frac{19}{150}\mu_1 + \frac{19}{50}\mu_2 + \frac{13}{24}\mu_3 + \frac{37}{100}\mu_4 + \frac{37}{300}\mu_5 \right) \right) \\ &+ e_4 \left(\frac{7}{75}\phi_1 + \frac{31}{150}\phi_2 - a^2 \left(\frac{7}{150}\mu_1 + \frac{7}{50}\mu_2 + \frac{7}{30}\mu_3 + \frac{161}{600}\mu_4 + \frac{31}{300}\mu_5 \right) \right) \\ &+ e_5 \left(\frac{1}{75}\phi_1 + \frac{13}{150}\phi_2 - a^2 \left(\frac{1}{150}\mu_1 + \frac{1}{50}\mu_2 + \frac{1}{30}\mu_3 + \frac{7}{150}\mu_4 + \frac{7}{200}\mu_5 \right) \right) \end{aligned} \right],$$

and central deflection from equation (23),

$$Y=a^2 \left[\begin{aligned} &e_1 \left(-\frac{13}{60}\phi_1 - \frac{1}{30}\phi_2 + a^2 \left(\frac{7}{80}\mu_1 + \frac{7}{60}\mu_2 + \frac{1}{12}\mu_3 + \frac{1}{20}\mu_4 + \frac{1}{60}\mu_5 \right) \right) \\ &+ e_2 \left(-\frac{31}{60}\phi_1 - \frac{7}{30}\phi_2 + a^2 \left(\frac{31}{120}\mu_1 + \frac{161}{240}\mu_2 + \frac{7}{12}\mu_3 + \frac{7}{20}\mu_4 + \frac{7}{60}\mu_5 \right) \right) \\ &+ e_3 \left(-\frac{9}{16}\phi_1 - \frac{9}{16}\phi_2 + a^2 \left(\frac{9}{32}\mu_1 + \frac{27}{32}\mu_2 + \frac{469}{384}\mu_3 + \frac{27}{32}\mu_4 + \frac{9}{32}\mu_5 \right) \right) \\ &+ e_4 \left(-\frac{7}{30}\phi_1 - \frac{31}{60}\phi_2 + a^2 \left(\frac{7}{60}\mu_1 + \frac{7}{20}\mu_2 + \frac{7}{12}\mu_3 + \frac{161}{240}\mu_4 + \frac{31}{120}\mu_5 \right) \right) \\ &+ e_5 \left(-\frac{1}{30}\phi_1 - \frac{13}{60}\phi_2 + a^2 \left(\frac{1}{60}\mu_1 + \frac{1}{20}\mu_2 + \frac{1}{12}\mu_3 + \frac{7}{60}\mu_4 + \frac{7}{80}\mu_5 \right) \right) \end{aligned} \right].$$

As an example of the application of the foregoing method to the purposes of calculation, let the case of the Britannia Bridge be taken, and let the large span be supposed to be divided into five, and the small span into three equal parts, and let the moments of inertia of the sections and loads per unit of length be supposed constant within each part and equal to their mean values.



We have then the following data:—
In spans (1 . 2) and (1 . 0),

$$\begin{aligned} a=92, \quad b=2a, \quad c=3a, \quad d=4a, \quad l=5a, \\ a'=76.7, \quad b=2a, \quad l'=3a; \\ e_1=\frac{1}{1132E}, \quad e_2=\frac{1}{1520E}, \quad e_3=\frac{1}{1746E}, \quad e_4=\frac{1}{1664E}, \quad e_5=\frac{1}{1857E}, \\ e_1'=\frac{1}{1100E}, \quad e_2'=\frac{1}{960E}, \quad e_3'=\frac{1}{720E}; \\ \mu_1=2.89, \quad \mu_2=3.31, \quad \mu_3=3.57, \quad \mu_4=3.49, \quad \mu_5=3.65, \\ \mu_1'=2.64, \quad \mu_2'=2.67, \quad \mu_3'=2.32; \\ T+T'=0, \quad E=1440000. \end{aligned}$$

In span (2 . 1),

$$\begin{aligned} a=92, \quad b=2a, \quad c=3a, \quad d=4a, \quad l=5a; \\ e_1=\frac{1}{1857E}, \quad e_2=\frac{1}{1664E}, \quad e_3=\frac{1}{1746E}, \quad e_4=\frac{1}{1520E}, \quad e_5=\frac{1}{1132E}; \\ \mu_1=3.65, \quad \mu_2=3.49, \quad \mu_3=3.57, \quad \mu_4=3.31, \quad \mu_5=2.89; \\ \text{and from symmetry of loading } T=\frac{1}{2}t=-0.002035. \end{aligned}$$

Applying equation (30) and (32) to spans (1 . 0) and (1 . 2) respectively, and eliminating T and T' by adding them, we obtain

$$0.1888\phi_1 + 0.04827\phi_2 - 10481 = 0;$$

and applying equation 32 to span (2 . 1),

$$0.04827\phi_1 + 0.08765\phi_2 - 5420 = 0,$$

whence $\phi_1 = 46206, \quad \phi_2 = 36387.$

Taking these values of ϕ_1 and ϕ_2 , and applying equation (33) to the calculation of the deflection at the middle of the large span,

$$Y = 0.375 \text{ ft.} = 4.5 \text{ inches.}$$

If, now, the values of ϕ_1 , ϕ_2 , and Y be calculated from equations (26) and (19), on the supposition that the moments of inertia of the section and the loads are constant throughout each span and equal to their mean values, they are

$$\phi_1 = 47030, \quad \phi_2 = 35610, \quad Y = 4.62,$$

which are almost identical with the values ascertained by Mr. Pole.

If the variation of section alone be considered, the load being taken at its mean value,

$$\phi_1 = 46382, \quad \phi_2 = 34465, \quad Y = 4.52.$$

It therefore appears that the amount of variation in the section and load which occurs in each span of the Britannia Bridge, when taken strictly into account, produces scarcely any effect on the values of the bending moments and deflections, which are practically the same as those resulting from their mean values considered as constant; and it may be

considered as demonstrated that, for most ordinary cases of large bridges, calculations founded on equation (26) may be confidently relied on. It need scarcely be remarked that these are much more simple and easy than those founded on the more exact but complex equations above given.

In smaller bridges, however, the error of the approximate process will be more considerable, and the process above given may be applied with advantage to its correction.

In concluding this paper, the author desires to record his thanks to his young friend, Mr. Henry Reilly, for the patience and skill with which he made, in detail, all the intricate calculations of the numerical values of the various functions involved in the above demonstration.

"Remarks on Mr. Heppel's Theory of Continuous Beams." By W. J. MACQUORN RANKINE, C.E., LL.D., F.R.S. Received December 22, 1869*.

1. *Condensed form of stating the Theory.*—The advantages possessed by Mr. Heppel's method of treating the mathematical problem of the state of stress in a continuous beam will probably cause it to be used both in practice and in scientific study.

The manner in which the theory is set forth in Mr. Heppel's paper is remarkably clear and satisfactory, especially as the several steps of the algebraical investigation correspond closely with the steps of the arithmetical calculations which will have to be performed in applying the method to practice.

Still it appears to me that, for the scientific study of the principles of the method, and for the instruction of students in engineering science, it may be desirable to have those principles expressed in a condensed form; and with that view I have drawn up the following statement of them, which is virtually not a new investigation, but Mr. Heppel's investigation abridged.

Let $(x=0, y=0)$ and $(x=l, y=0)$ be the coordinates of two adjacent points of support of a continuous beam, x being horizontal. Let y and the vertical forces be positive downwards.

At a given point x in the span between those points let μ be the load per unit of span, and EI the stiffness of the cross-section, each of which functions may be uniform or variable, continuous or discontinuous.

In each of the following double and quadruple definite integrals, let the lower limits be $x=0$.

$$\left. \begin{aligned} \iint \mu dx^2 &= m; & \iint \frac{dx^2}{EI} &= n; \\ \iint \frac{x dx^2}{EI} &= q; & \iint \frac{dx^2}{EI} \iint \mu dx^2 &= F. \end{aligned} \right\} \dots \dots (1)$$

When the integrations extend over the whole span l , that will be denoted by affixing 1; for example, m_1, n_1 , &c.

* Read January 27, 1870. See vol. xviii. p. 178.

moment,	$\Phi = \Phi_0 - Px + m;$	(2)
deflection,	$y = Tx - Pq + \Phi_0 \pi + F.$	(3)

$$\Phi = \Phi_0 - Px + m; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$y = Tx - Pq + \Phi_n \pi + F, \quad (3)$$

$$P = \frac{\Phi_0 - \Phi_1 + m_1}{l}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$
$$T = \frac{Pq_1 - \Phi_1 n_1 - F_1}{l} = \Phi_1 \left(\frac{q_1}{l} - \frac{n_1}{l} \right) - \frac{\Phi_1 q_1}{l^2} + \frac{n_1 q_1}{l^2} - \frac{F_1}{l}. \quad (5)$$
$$-T^* = \Phi_* \left(\frac{q_{-1}}{l^2} - \frac{n_{-1}}{l} \right) - \frac{\Phi_{-1} q_{-1}}{l^2} + \frac{m_{-1} q_{-1}}{l^3} - \frac{P_{-1}}{l}. \quad (5A)$$
$$0 = \Phi_0(q_1 l^2 + q_{-1} l^2 - n_1 l l^2 - n_{-1} l l^2) - \Phi_1 q_1 l^2 - \Phi_{-1} q_{-1} l^2 \left. \begin{array}{l} + m_1 q_1 l^2 + m_{-1} q_{-1} l^2 - F_1 l l^2 - F_{-1} l l^2 - l l l^2. \end{array} \right\} \quad (6)$$

That equation gives a linear relation between the bending moments $\Phi_{-1}, \Phi_0, \Phi_1$ at any three consecutive points of support, and certain known functions of known quantities. In a continuous girder of N spans there are $N-1$ such equations and $N-1$ unknown moments; for the moments at the end most supports are each $=0$. The moments at the intermediate points of support are to be found by elimination; which having been done, the remaining quantities required may be computed for any particular span as follows:—The inclination T at a point of support by equation (5); the shearing force P at the same point by equation (4); the deflection y and moment Φ at any point in that span by equations (3) and (2). Points of maximum and minimum bending moment are of course found by making $\frac{d\Phi}{dx}=0$; and points of inflection by making $\Phi=0$.

2. *Case of a uniform girder with an indefinite number of equal spans, uniformly loaded; loads alternately light and heavy.*—The supposition just described forms the basis of the formulæ given in a treatise called 'A Manual of Civil Engineering,' page 288; and it therefore seems to me desirable to test those formulæ by means of Mr. Heppel's method.

The cross-section of the whole girder and the load on a given span being uniform, the definite integrals of the formulæ (1) take the following values:—

$$m = \frac{\mu x^3}{2}; \quad n = \frac{x^3}{2EI}; \quad q = \frac{x^3}{6EI}; \quad F = \frac{\mu x^4}{24EI} = \frac{mq}{2x} \dots \dots (7)$$

The values of those integrals for the complete span are expressed by making $x=l$.

The values of n and q are the same for every span. In the values of m and F , the load μ per unit of span has a greater and a less value alternately. Let w_0 be the weight per unit of span of the girder, with its fixed load, w_1 that of the travelling load (increased, if necessary, to allow for the additional straining effect of motion); then the alternate values of μ are

$$\mu = w_0; \quad \mu' = w_0 + w_1 \dots \dots \dots (8)$$

The moments at the points of support are all equal; that is, $\Phi_0 = \Phi_1 = \Phi_{-1}$.

Equation (6) now becomes the following (the common factor l^3 having been cancelled):—

$$0 = -2\Phi_0 n_1 + F_1 + F_{-1} - tl;$$

giving for the bending moment at each point of support

$$\Phi_0 = \frac{F_1 + F_{-1} - tl}{2n_1} = \frac{2w_0 + w_1}{24} \cdot l^2 - \frac{t}{l} EI \dots \dots \dots (9)$$

If t be made $=0$, so that the continuity is perfect, this equation exactly agrees with the formula at page 289 of the treatise just referred to; and the same is the case with the following formulæ for the shearing-forces and slopes close to a point of support, and for the moments and deflections at other points:—

$$\left. \begin{array}{l} \text{Shearing-force, light load,} \quad P = \frac{w_0 l}{2}; \\ \text{Shearing-force, heavy load,} \quad P_1 = \frac{w_0 + w_1}{2} \cdot l. \end{array} \right\} \dots \dots \dots (10)$$

$$\left. \begin{array}{l} \text{Slope, light load,} \quad T = \frac{Pq_1 - \Phi_0 n_1 - F_1}{l} = \frac{t}{2} - \frac{w_0 l^3}{48EI}; \\ \text{Slope, heavy load,} \quad -T = \frac{t}{2} + \frac{w_1 l^3}{48EI}. \end{array} \right\} \dots \dots \dots (11)$$

Moment, light load,

$$\Phi = \Phi_0 - Px + m = -\frac{t}{l}EI + \frac{2w_0 + w_1}{24}l^3 - \frac{w_0 lx}{2} + \frac{w_0 x^2}{2};$$

Moment, heavy load,

$$\Phi' = -\frac{t}{l}EI + \frac{2w_0 + w_1}{24}l^3 - \frac{w_0 + w_1}{2}lx + \frac{w_0 + w_1}{2}x^2.$$

$$\left. \begin{array}{l} \text{Central moment, light load, } \Phi\left(x = \frac{l}{2}\right) = -\frac{t}{l}EI + \frac{w_1 - w_0}{24}l^3; \\ \text{Central moment, heavy load, } \Phi'\left(x = \frac{l}{2}\right) = -\frac{t}{l}EI - \frac{w_0 + 2w_1}{24}l^3. \end{array} \right\} \quad (12)$$

Central deflection, light load,

$$y = Tx - Pq + \Phi_0 n + F\left(\text{with } x = \frac{l}{2}\right) = \frac{tl}{8} + \frac{w_0 - 2w_1}{384EI}l^4;$$

Central deflection, heavy load,

$$y = -Tx - P'q + \Phi_0 n + F\left(\text{with } x = -\frac{l}{2}\right) = \frac{tl}{8} + \frac{w_0 + 3w_1}{384EI}l^4.$$

COMMUNICATIONS RECEIVED SINCE THE END OF THE SESSION.

“Researches into the Chemical Constitution of the Opium Bases.
—Part IV. On the Action of Chloride of Zinc on Codeia.” By
AUGUSTUS MATTHIESSEN, F.R.S., Lecturer on Chemistry at St.
Bartholomew's Hospital, and W. BURNSIDE, of Christ's Hos-
pital. Received June 23, 1870.

On endeavouring to prepare apomorphia by a cheap method, Mr. Mayer and one of us heated morphia with chloride of zinc, to see whether the elements of water could not be abstracted by this reagent (the results of this reaction have not yet been published). Apomorphia having been obtained in this manner, it seemed possible that apocodeia, that is codeia minus the elements of water, might be prepared by a similar reaction. On trying the experiment a new base was obtained, which proved on analysis to be apocodeia.

When hydrochlorate of codeia is heated with an excess of a concentrated solution of chloride of zinc, to a temperature varying between 170° and 180° C., for about 15 minutes, the decomposition takes place; on cooling a yellowish-brown tarry mass separates from the liquid, which on further cooling may be drawn into thin threads, and thus obtained almost free from the excess of the chloride of zinc. This amorphous silk-like mass is almost pure hydrochlorate of apocodeia. To obtain the base in a pure state from this substance, the following method was employed:—

The hydrochlorate was dissolved in hot water and precipitated by hy-

drochloric acid. The liquid containing the precipitated hydrochlorate was allowed to cool, and the precipitate on solidifying was separated from the acid solution. The operation of dissolving and reprecipitating with hydrochloric acid was repeated several times, and lastly the hydrochlorate was dissolved in water, precipitated with carbonate of sodium, and the base extracted with ether. On evaporating the ether-solution the base remained behind as an amorphous, gum-like, reddish mass; this was powdered, dried in a water-bath, and gave on analysis the following results. All combustions were made with oxide of copper and oxygen.

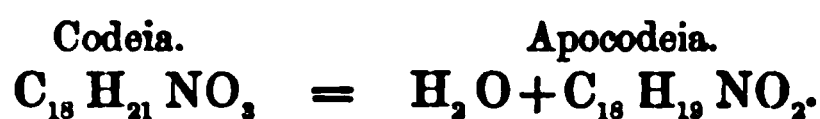
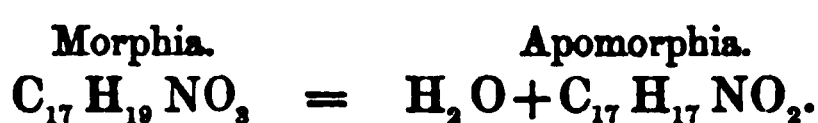
(I.) 0.3245 gramme of the base, dried at 100° C., gave 0.9150 carbonic acid and 0.2080 water.

(II.) 0.3150 gramme of the base gave 0.8860 carbonic acid and 0.1960 water.

(III.) 0.4570 gramme of the base, burnt with soda-lime, gave 0.1600 metallic platinum.

			Found.		
			(I.)	(II.)	(III.)
C ₁₈	216	76.87	76.89	76.70	
H ₁₉	9	6.76	7.12	6.91	
N	14	4.98			4.97
O ₂	32	11.39			
	<u>281</u>	<u>100.00</u>			

The reaction that has therefore taken place is similar to that of hydrochloric acid on morphia, viz. that the chloride of zinc has abstracted the elements of water, thus:—



The base itself is soluble in alcohol, ether, and chloroform, but almost insoluble in water, and has not yet been obtained in the crystalline state.

The hydrochlorate, obtained by shaking the ether-solution of the pure base with hydrochloric acid, and evaporating the acid solution to dryness, gave the following on analysis:—

0.563 gramme of the hydrochlorate gave 0.256 chloride of silver.

		Calculated.	Found.
C ₁₈ H ₂₀ NO ₂	282	88.82	
Cl	35.5	11.18	11.25
	<u>317.5</u>	<u>100.00</u>	

The hydrochlorate cannot be obtained in a crystalline state; it is easily soluble in water, and is precipitated thence by strong hydrochloric acid.

On comparing the actions of different reagents on this base with those obtained with apomorphia (Proc. Roy. Soc. No. 112, 1869, p. 459), they

were found to be almost identical, the most marked of the few differences being that the blood-red colour given with nitric acid is much more permanent than in the similar apomorphia reaction. Between the two bases also a very marked difference exists in respect of stability, apocodeia being far superior in this respect to apomorphia; in fact it may be precipitated by ammonia or carbonate of sodium, washed and dried, without undergoing a marked change of colour.

The hydrochlorates also differ; for that of apomorphia can be easily crystallized, whereas hydrochlorate of apocodeia has only been obtained in an amorphous state. The preparation of apocodeia is easy and sure, yielding a very large product. In this respect it differs materially from apomorphia, the preparation of which is tedious, and the amount of yield very uncertain, hence the high price of this valuable therapeutical agent. The solutions of the two hydrochlorates also show the same differences that the bases themselves do. In physiological effects also there is a decided difference between the hydrochlorates, that of apomorphia being, as observed by Dr. Gee, a very violent emetic, whilst that of apocodeia is, according to Dr. Legg's experiments, a mild emetic; it also produces subcutaneous abscesses at the place of injection, which the apomorphia salt does not.

It has been shown in Part II. (Proc. Roy. Soc. vol. xvii. p. 460) of these researches, that when codeia is heated with hydrochloric acid it splits up into chloride of methyl, water, and apomorphia. The action of hydriodic acid on narcotine for the elimination of the methyl contained in it is, however, more energetic than that of hydrochloric acid. Therefore it was thought probable that, by means of hydriodic acid, CH_3 might be abstracted alone, as iodide of methyl, from the codeia, leaving the elements of water, and thus forming morphia.

On trying the experiment, however, not a trace of iodide of methyl was obtained, but the iodide of a new base, which is at present under examination.

The codeia with which the foregoing experiments were made was kindly presented to us by Messrs. McFarlan and Co., of Edinburgh, to whose liberality we are already so much indebted.

"Experiments on the Action of Red Bordeaux Wine (Claret) on the Human Body." By E. A. PARKES, M.D., F.R.S., Professor of Hygiene in the Army Medical School, and Count CYPRIAN WOLLWICZ, M.D., Assistant Surgeon, Army Medical Staff.
Received July 5, 1870.

In the Proceedings of the Royal Society (No. 120) is an account of some experiments with pure alcohol and brandy on a healthy man. This paper is intended as a continuation, with the substitution in the experiments of red Bordeaux wine (claret) for alcohol and brandy. The same

man was the subject of the experiments, and he was placed on precisely the same diet as is recorded in the former paper.

The experiments were continued for 30 days, the man having abstained from any alcoholic beverage for 16 days previously. During the first 10 days, water only was taken at dinner, during the next 10 days red Bordeaux wine was substituted for the water; 10 fluid ounces (284 cub. centims.) being given on the first 5 days, and 20 fluid ounces (568 cub. centims.) on the last 5 days. The wine was taken at dinner time, at a quarter past 1 o'clock. In the last 10 days water was again given.

The wine was a good claret, as it was thought best to use a superior wine; it was Haut Brion wine of second growth, of the vintage of 1863, and was sold in London at the price of 60s. per dozen. It contained 11 per cent. of alcohol. The free acidity was equal to about 3 grains per ounce of tartaric acid ($C_4H_6O_6$); the total solids amounted to 21.76 grammes, and the fire-proof salts to 2.359 grammes per litre. Of this amount of salts 2.027 grammes were soluble, and .332 insoluble. In the former, phosphoric acid and chlorine were present in the amounts of .145 and .106 gramme per litre respectively; the insoluble salts contained only .0175 gramme of phosphoric acid per litre. In the 10 ounces of wine there were therefore only 0.7 grain of phosphoric acid, and 0.46 grain of chlorine.

The ash was intensely alkaline, and, when neutralized with standard acid, the alkalinity was found to be equal to 1.679 gramme of tartaric acid ($C_4H_6O_6$) per litre.

Only two circumstances (except the taking of wine) were different in this set of experiments as compared with the former.

The first experiments were made in February and March 1870, when the weather was very cold; the present were made in May and June in very hot and dry weather. The only influence we could trace to this altered condition of climate was that the amount of water allowed was insufficient, and the man suffered some discomfort from thirst. We could not perceive that any effect was produced on the nitrogenous elimination; certainly there was no diminution.

The other alteration was that the man had gained 4 lbs. in weight, and was still gaining a little when the experiments were commenced; he continued to do so slowly until the 24th day, when his health began to give way and he lost weight.

The experiments included the number of the pulse (taken in the recumbent position) every 2 hours from 8 A.M. to 10 P.M., tracings of the pulse and respirations, the temperature of the axilla every 2 hours from 6 A.M. to 10 P.M., the temperature of the rectum four times a day (the observations being taken with the same thermometers as on the former occasion), the amounts of nitrogen, phosphoric acid, chlorine and free acidity of the urine, and the weight, and in the two cases the amount of nitrogen in the stools.

1. WEIGHT OF THE BODY

(taken at 8 a.m. before breakfast and after emptying the bladder).

Days.	Weight, in lbs.	Weight, in kilo- grammes.	Days.	Weight, in lbs.	Weight, in kilo- grammes.	Days.	Weight, in lbs.	Weight, in kilo- grammes.
1.	140	63.6	11.	140.5	63.86	21.	140.5	63.86
2.	140	63.6	12.	140.5	63.86	22.	140.6	63.91
3.	140	63.6	13.	140.6	63.91	23.	140.6	63.91
4.	140	63.6	14.	140.6	63.91	24.	140.6	63.91
5.	140	63.6	15.	140.6	63.91	25.	140.6	63.91
6.	140.5	63.86	16.	140.6	63.91	26.	140.4	63.81
7.	140.5	63.86	17.	140.6	63.91	27.	140.3	63.77
8.	140.5	63.86	18.	140.6	63.91	28.	140	63.6
9.	140.5	63.86	19.	140.5	63.86	29.	140	63.6
10.	140.5	63.86	20.	140.5	63.86	30.	140	63.6
Means	140.25	63.75	140.55	63.863	140.35	63.79

Owing to the rather larger supply of food and the lessened exercise the weight increased slightly, but remained, on the whole, in tolerable equilibrium until the 24th day, when he became indisposed, and lost weight regularly every day for 4 days. No obvious change in weight was caused by the wine.

2. THE CIRCULATION.

Pulse before wine (taken in the recumbent position.)

Days.	Hours.								Mean of the days.
	8 A.M.	10 A.M.	12 noon.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	
1st day ..	74	86	74	84	77	84	72	72	77.87
2nd day ..	67	72	72	82	77	80	75	70	74.37
3rd day ...	71	80	72	82	76	82	85	72	77.5
4th day ..	65	75	80	89	73	73	78	67	75
5th day ..	76	82	72	84	81	82	72	73	77.87
6th day ...	76	74	77	87	73	76	72	72	76.0
7th day ...	72	78	70	90	74	75	90	69	77.2
8th day ...	76	79	73	83	78	71	70	72	76.87
9th day ...	67	76	75	77	75	76	73	73	74.2
10th day...	73	80	77	79	74	76	78	72	75.5
Mean	71.7	78.4	74.2	83.7	75.8	78.8	77.1	71.3	76.3

The pulse in this man had a daily course of great uniformity, the changes being chiefly dependent on food and in a less degree on exercise. If the last line (the mean of the hours) is read, and it is remembered that breakfast was taken at 8, dinner at 1, and tea at 5, the increase in the number of beats at 10, 2, and 6 o'clock is at once accounted for. It rose after breakfast nearly 7 beats; then fell 4 beats; rose after dinner nearly 10 beats; then fell, but not to its previous standard; rose after tea 3 beats, and then fell, till at 10 P.M. it was nearly the same as at 8 A.M. The

other cause influencing the heart's beats was exercise; we kept the exercise as uniform as we could, but there were variations, and we could often trace defect or excess of exercise on the next reading of the pulse. The daily mean of the pulse was fairly uniform, the mean of the 10 days being 76·3 beats per minute, the extreme mean daily variation was from 74·2 to 77·87.

Pulse during wine; 10 ounces at 1 o'clock during the first 5 days, and 20 ounces during the last 5.

Days.	Hours.								Mean of the days.
	8 A.M.	10 A.M.	12 noon.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	
11th day...	87	79	76	79	80	87	80	72	77·5
12th day...	72	71	72	85	82	80	95	82	81·1
13th day...	76	78	70	86	84	89	80	73	78·8
14th day...	67	82	83	92	87	89	76	78	81·7
15th day...	70	81	77	92	88	93	84	70	82·6
16th day...	77	80	75	76	94	86	87	76	81·3
17th day...	74	82	75	93	88	86	80	74	81·5
18th day...	76	75	79	94	88	91	78	69	80·7
19th day...	76	82	69	86	96	89	82	78	82·2
20th day...	68	86	67	85	89	81	79	71	78·2
Means.....	72·3	79·1	73·9	86·8	87·6	88·1	82·1	74·9	80·5

The wine increased the frequency of the heart's action by $4\frac{1}{2}$ beats every minute during 14 hours in the day, and doubtless also in the remaining 10, for the pulse at 8 A.M. was still too frequent during the wine period. In the 24 hours there was then an excess in the heart's action of 6120 beats, or nearly 6 per cent. As the amount of alcohol was 1·1 ounces in the first 5 days, and 2·2 ounces in the other 5, the increase in the number of the heart's beats was slightly more than in the days when an equal quantity of pure alcohol was taken.

This was partly owing to the continuance of the wine, as the first day's excess was only 1658 beats, and partly to the fact that whereas in the former series of experiments the mean pulse-beats in the water period were 73·, in this they were 76·3. The man's heart was evidently rather more excitable in this series than in the former.

When the hourly changes are compared with the water period, it is seen that the influence of food is marked as before, but that the wine exaggerated the effect, and kept the pulse at a greater rate for a longer time.

An extract from the Tables will show this. It must be noted that the wine was taken at 1 o'clock, or a little after.

	Water period.	Wine period.
Mean number of pulse at 10 A.M. after breakfast..	78·4	79·1
Mean at 2 P.M. after dinner	88·7	86·8
Mean at 4 P.M.....	75·8	87·6
Mean at 6 P.M. after tea	78·8	88·1
Mean at 8 P.M.....	76·6	82·1
Mean at 10 P.M.	71·3	74·9

It will be seen, then, that the pulse at 4, 6, and 8 o'clock in the wine period is much above the corresponding numbers in the water period. The effect of the wine is largely perceptible for eight hours, and is traceable during all the observations. The mean of the first five days is 80.34 beats per minute, and of the last five days 80.78 beats.

The effect of increasing the wine to 20 ounces is chiefly perceived in the greater acceleration of the pulse at 4 o'clock in the last five days as compared with the first five. When 10 ounces were taken, the mean pulse at 4 o'clock was 84.2, or two beats per minute less than at 2 o'clock, whereas in the 20-ounce days the mean pulse at 4 o'clock was four beats above the 2 o'clock rate.

Pulse after wine.

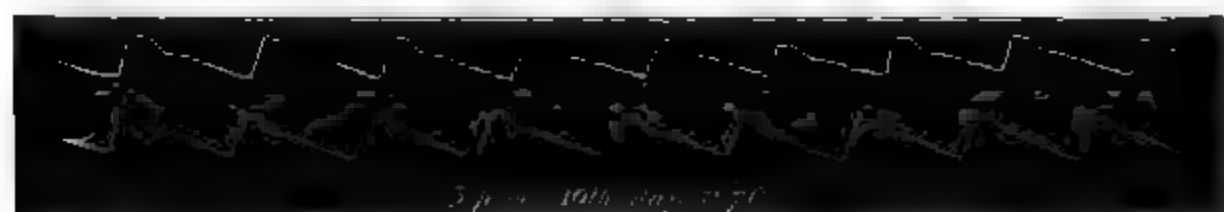
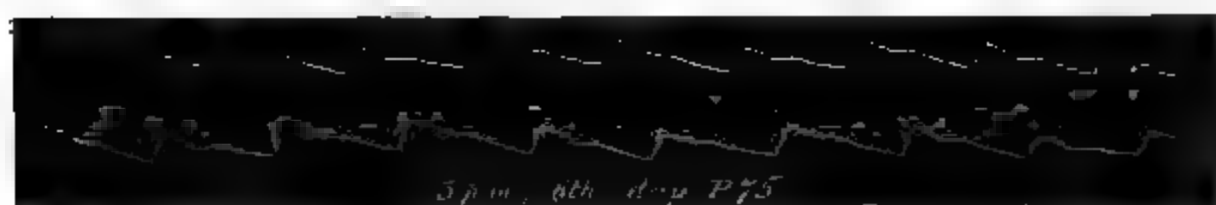
Days.	Hours.								Mean of the days.
	8 A.M.	10 A.M.	12 noon.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	
21st day ...	68	86	74	96	84	90	70	69	79.6
22nd day...	78	84	72	80	76	83	81	69	78.1
23rd day...	72	80	74	81	84	78	81	72	78.1
24th day...	73	79	76	83	79	84	82	74	78.75
25th day...	70	77	73	77	74	82	81	73	76.5
26th day...	69	84	77	82	78	81	77	65	77
27th day...	70	75	95	99	89	92	92	80	84.5
28th day...	70	79	87	86	84	86	84	84	82.5
29th day...	74	82	74	84	84	80	83	79	81
30th day...	70	80	70	96	89	79	74	72	78.75
Means.....	71.4	80.6	77.2	86.7	82.3	83.8	80.5	74.2	79.38

The pulse continued high during the whole of this period, the excess being chiefly in the afternoon hours; even 10 days after the wine was left off it had not returned to its proper rate; but this was probably in part owing to indisposition, which will be referred to presently.

Sphygmographic observations were taken three times a day; but as the curves from alcohol were so fully given in the former paper, we have thought it necessary only to put in nine curves, three before, four during, and two after wine. We have selected 3 o'clock as the hour, so that the influence of food is perceptible in all: the effect of the wine was the same as that of alcohol, though of course in a degree proportional to the amount.

We also attempted to determine the ratio of the radial pulse, heart's action, and respiration by means of Dr. Burdon-Sanderson's ingenious cardiograph. Unfortunately we did not obtain the instrument in time to determine the curves properly in the period before wine, and we are therefore not able to give proper comparisons. We could not, however, so far trace any effect on the number or depth of the respirations.

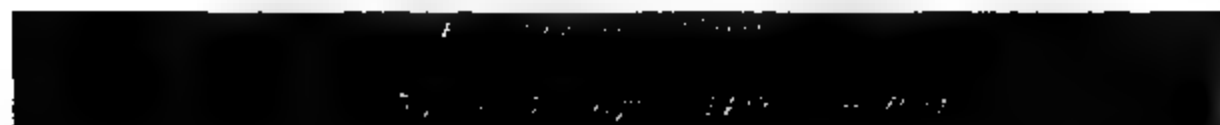
Before claret.—At 3 p.m., 2 hours after dinner.



During claret.—2 hours after dinner.



After claret.—2 hours after dinner.



3. THE TEMPERATURE OF THE BODY

The temperature was taken both in the axilla and rectum, in order to obtain a control of the observations. The degrees are Fahrenheit.

(a) In the Axilla.

The thermometer was kept in the axilla for 20 minutes or more, while the man was in bed and covered with the clothes.

First Period. Temperature of axilla before wine.

Days.	Hours.									Mean of the days.
	6 A.M.	8 A.M.	10 A.M.	12 noon.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	
1st day ...	97.8	98.0	98.0	98.0	98.4	98.4	98.4	98.2	98.4	98.17
2nd day ...	97.4	97.4	98.0	97.6	98.4	97.8	98.0	98.8	98.0	97.93
3rd day ...	97.8	97.0	97.4	97.4	98.2	98.0	97.2	98.6	98.4	97.77
4th day ...	97.2	97.0	98.4	97.3	97.4	97.4	97.8	98.0	97.6	97.56
5th day ...	97.8	97.4	97.2	97.0	97.6	97.6	98.0	98.0	97.6	97.57
6th day ...	97.6	97.4	97.6	97.8	97.6	97.8	97.6	97.6	97.2	97.57
7th day ...	97.6	97.8	97.4	97.0	98.0	97.6	97.8	97.6	97.4	97.57
8th day ...	97.8	97.6	97.2	97.8	97.8	97.4	97.6	98.0	97.4	97.62
9th day ...	97.4	97.2	98.2	97.6	98.2	97.6	98.0	97.4	98.0	97.73
10th day...	97.0	97.4	98.0	98.0	98.2	97.4	97.8	98.8	97.4	97.77
Means.....	97.54	97.42	97.74	97.55	97.98	97.70	97.82	98.10	97.74	97.726

It will be seen on reading the last line (mean of the hours) that the temperature follows the same course as the pulse in being manifestly influenced by food, and rising after breakfast, dinner, and tea. The only exception (and this is perhaps apparent only) is at 8 P.M., when the mean temperature is higher than at 6 P.M. while the pulse is falling; but this was perhaps accidental, i. e. a longer series of observations might have given different results; for in three observations the temperature was higher at 6 o'clock, and in three it was equal, while in the other four, when it was highest at 8 o'clock, there were two exceptional high temperatures which augmented the mean amount. In the next period the mean temperature at 8 P.M. was lower than at 6.

We were unable to see any diurnal change of temperature in this man apart from food; there was no afternoon or evening rise of temperature, dependent solely on the time of day.

The temperature was more uniform than in the experiments in February,

Second Period. Temperature of axilla during wine.

Days.	Hours.									Mean of the days.
	6 A.M.	8 A.M.	10 A.M.	12 noon.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	
11th day...	97.0	97.2	97.6	97.8	98.0	97.6	97.6	97.8	97.4	97.55
12th day...	97.2	97.4	97.0	97.4	97.0	97.2	98.0	97.6	97.4	97.33
13th day...	97.0	97.2	97.6	97.6	98.0	97.4	98.2	97.4	97.0	97.48
14th day...	96.8	97.0	97.8	97.2	98.0	97.6	98.4	98.0	97.6	97.4
15th day...	97.4	97.6	98.0	97.4	98.2	98.2	98.0	98.2	97.8	97.85
16th day...	97.4	97.8	97.4	98.0	97.8	98.0	98.0	97.6	97.6	97.73
17th day ..	97.8	97.0	97.4	97.4	98.0	97.8	97.4	97.0	97.0	97.31
18th day..	97.2	97.2	97.6	98.0	97.8	97.6	97.4	not taken		97.54
19th day...	97.6	98.0	97.4	97.4	98.2	97.8	98.0	97.8	98.0	97.8
20th day...	96.8	97.0	98.0	97.8	98.0	97.4	97.8	97.6	97.8	97.69
Means.....	97.19	97.3	97.58	97.60	97.90	97.68	97.88	97.66	97.50	97

If the mean temperature at 2, 4, 6, and 8 o'clock, when the wine was acting most on the pulse, are placed side by side, we have,—

Hours.	Temperature.	
	Water period.	Wine period.
2 P.M.	97.98	97.90
4 P.M.	97.70	97.68
6 P.M.	97.82	97.68
8 P.M.	98.10	97.66

The temperatures of the three first hours are practically identical, and as already said, the rise at 8 o'clock in the water period seems to us accidental, i. e. as dependent on two exceptional high temperatures, which raised the mean amount. In the other 5 hours the mean temperature was four times slightly higher in the water, and once in the wine period.

The result of all the observations was that, in the water period of ten days, the mean temperature was $97^{\circ}.726$, and in the wine period was $97^{\circ}.56$, or $0^{\circ}.166$ less, a difference so slight as probably to fall within the limits of unavoidable error. The mean of the first five days, with 10 ounces of wine, was $97^{\circ}.526$; the mean of the last five days, with 20 ounces of wine, was $97^{\circ}.590$, proving that doubling the amount of wine caused no lowering of mean temperature, and probably no rise, as the difference is so slight.

We conclude that in health the apparent heat after wine must be owing, as in the case of alcohol and brandy, rather to subjective feelings connected with the quickened circulation than with an actual rise of temperature; but that, on the other hand, wine in the above quantities causes no appreciable lowering of temperature.

Third Period. Temperature of Axilla after wine.

Days.	Hours.									Mean of the days.
	6 A.M.	8 A.M.	10 A.M.	12 noon.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	
21st day ...	97.2	97.4	98.0	98.3	99.2	98.4	98.6	97.6	97.6	■
22nd day ...	97.4	97.4	98.2	97.8	98.2	97.8	98.0	97.4	97.0	97.6
23rd day ...	97.6	97.6	98.0	98.0	98.4	98.0	97.8	98.0	98.2	97.9
24th day ...	97.2	97.4	97.6	97.6	98.2	97.6	98.0	97.6	97.0	97.5
25th day ...	97.8	98.0	97.8	98.4	■	98.0	98.2	97.6	97.2	98.0
26th day ...	97.0	97.0	98.2	98.4	98.8	97.6	97.6	97.8	97.4	97.7
27th day ...	97.0	96.8	97.8	98.4	98.8	98.6	98.0	97.8	97.0	97.8
28th day ...	97.0	97.0	97.4	98.0	98.6	98.2	98.0	97.8	97.6	97.7
29th day ...	97.2	97.0	97.6	97.6	98.0	97.6	98.0	98.4	98.2	97.7
30th day ...	97.4	97.6	98.0	97.4	98.2	97.8	98.0	97.8	97.6	97.5
Means.....	97.28	97.32	86	97.99	98.66	97.96	98.04	97.76	97.48	97.55

In this period the diurnal variations were almost identical with the others, and the mean temperature of the whole period was practically the same as that of the first ten days.

(6) In the Rectum.

Rectum before wine (thermometer inserted for about 3 inches, and kept in for 20 minutes).

Days.	Hours.				Mean of the days.
	8 A.M.	12 noon.	4 P.M.	10 P.M.	
1st day ...	99	99.6	99.95	99.8	99.27
2nd day ...	99.2	99.2	99.4	99.2	99.25
3rd day ...	99.8	99	99.2	99.5	99.37
4th day ...	99.8	99.4	99.4	99.6	99.55
5th day ...	99.4	99.6	99.2	99	99.3
6th day ...	99.2	99.8	99.6	99.6	99.55
7th day ...	99.6	99.6	99.2	99.4	99.45
8th day ...	99.4	99.6	99.4	99.2	99.4
9th day ...	99	99.4	98.6	99.0	99
10th day...	98.6	99.8	99.4	99.8	99.65
Means ...	99.2	99.50	99.29	99.41	99.38

The mean temperature of the rectum (taken four times a day instead of three, as in the former experiments, and at different hours) was rather higher than the mean of the former experiments, viz. as 99.38 to 99.066. It was also more uniform, both from day to day and hour to hour. If these four hours be accepted as giving the mean temperature of the 24 hours, the rectum temperature was 1°·654 above that in the axilla.

Rectum during wine.

Days.	Hours.				Mean of the days.
	8 A.M.	12 noon.	4 P.M.	10 P.M.	
11th day..	98.6	99.9	99.2	99.6	99.37
12th day .	98.4	99.4	99.6	99.2	99.15
13th day..	98.6	99.4	99.8	99	99.2
14th day..	98.8	99.8	99.2	99	99.2
15th day	99.4	99.6	99	99	99.25
16th day	99.8	99.4	99.6	99.4	99.55
17th day .	98.6	99.6	99.8	99	99.25
18th day...	98.8	99.6	99.2	98	98.9
19th day...	98.6	99.5	99	98.4	98.87
20th day...	99	99.8	99.4	99.8	99.5
Means ...	98.81	99.60	99.38	99.04	99.22

The mean temperature is 0°·16 lower. It is curious that this is almost precisely the same change as in the case of the axillary temperature; yet it is probably an accidental coincidence. The 4 P.M. temperature, which ought to show the effect of wine, is slightly higher (0°·09) than in the first period; the 10 P.M. and 8 A.M. temperatures are lower by nearly 0°·3, and the 12 o'clock temperature is higher by 0°·1.

The differences are thus slight, and in contrary directions, so that no decided influence, one way or the other, can, we think, be ascribed to the wine.

Rectum after wine.

Days.	Hours.				Mean of the days.
	8 A.M.	12 noon.	4 P.M.	10 P.M.	
21st day ...	99.8	99.9	99.4	99.2	99.57
22nd day ..	99.0	99.6	99.8	99.0	99.45
23rd day..	99.0	99.5	99.2	99.6	99.32
24th day ..	99.2	99.4	99.2	99.0	99.2
25th day...	99.6	99.4	99.4	99.2	99.4
26th day...	98.8	99.2	99.4	99.0	99.1
27th day...	98.8	99.2	99.2	99.4	99.15
28th day...	99.0	99.4	99.6	99.4	99.35
29th day...	99.6	99.2	99.0	99.6	99.35
30th day .	98.8	99.8	99.6	99.4	99.4
Means.....	99.16	99.46	99.38	99.28	99.30

The temperatures are almost precisely the same as in the first period. The 4 o'clock temperature is identical with that of the wine-period.

4. ACTION ON THE URINE.

Elimination of water by the kidneys.

Twenty-eight fluid ounces were taken as drink, and the water in the so-called solid food made the total daily ingress of water $72\frac{1}{2}$ fluid ounces, or 2059 cub. centims.

The following are the means of the three periods:—

	Amount of water taken daily in solid food and as drink..	Mean amount of urine passed in 24 hours.
1st period (water)	2059 c.c.	1210 c.c.
2nd period (wine)	2010 c.c.	1148 c.c.
3rd period (water).....	2059 c.c.	1155 c.c.

As 49 cub. centims. less water were taken in the wine-period, the amount of urine ought perhaps to be increased by this amount, and this would make it only 13 cub. centims. less than the first period.

It may be concluded that 10 and 20 ounces of light wine (containing 1.1 and 2.2 ounces of alcohol), when substituted for water, had no diuretic effect. The amount of alcohol to act as a diuretic was perhaps too small, as in the former series with the larger quantities of alcohol there was certainly some increased flow of urinary water.

Elimination of nitrogen by the kidneys.

The same amount of food being given as in the previous experiments, the amount of nitrogen passing into the body was $17\frac{1}{2}$ or $17\frac{1}{2}$ grammes, or probably a little more. The whole of this passed by the urine and bowels, so that in this respect the difference in the temperature of the air had no effect. In other words, although the weather was so hot, there was no evidence of urea escaping by the skin.

The substances precipitated by Liebig's mercuric nitrate were as usual termed urea, and the nitrogen was calculated from this. It was also for the sake of control determined by soda-lime.

Nitrogen before claret.

Days.	Urea.	Nitrogen calculated from urea.	Nitrogen by soda-lime.
	grammes.	grammes.	grammes.
1st day	30.299	14.139	14.211
2nd day	33.343	15.560	16.555
3rd day	31.004	14.487	14.917
4th day	34.960	16.315	16.933
5th day	31.038	14.484	15.323
6th day	40.200	18.760	18.639
7th day	37.800	17.940	17.469
8th day	39.633	18.495	18.024
9th day	37.050	17.290	16.779
10th day.....	39.940	18.635	
Means	35.535	16.680	16.539

The mean of the nine days of ureal nitrogen, which correspond with the days of soda-lime nitrogen, is 16.493 grammes. The mean of the ten first days in the previous series, with an equal quantity of food, was 16.211 grammes of nitrogen as calculated from the urea, and 16.226 grammes as determined by soda-lime. In the present experiments the amounts are higher in a very trifling degree, viz. .379 gramme, and .313 gramme in excess respectively. The difference is so slight (under 6 grains in 24 hours) that the two series may be considered identical. Possibly as the man was 4 lbs. heavier, there might be some additional nitrogenous tissue furnishing the slight excess of nitrogen.

Nitrogen during claret.

Days.	Urea.	Nitrogen calculated from urea.	Nitrogen by soda-lime.
	grammes.	grammes.	grammes.
11th day.....	37.137	17.331	
12th day.....	35.325	16.485	16.539
13th day.....	38.356	17.899	18.825
14th day.....	34.860	16.268	16.074
15th day.....	35.040	16.352	17.255
16th day.....	37.570	17.532	16.707
17th day.....	41.745	19.486	18.886
18th day.....	35.048	16.355	15.764
19th day.....	34.650	16.170	15.443
20th day.....	31.510	14.701	14.600
Means	36.124	16.858	16.421

The variations from the period before claret are so slight, and indeed

insignificant, as to prove that 10 and 20 fluid ounces of claret, taken for two periods of five days, caused no alteration in the elimination of nitrogen, when the egress of nitrogen was constant.

Thus to express the result in grains, the daily nitrogen calculated from the urea was, in the first period of ten days, 257·37 grains, and in the second or wine-period 260 grains. In nine days of the two periods, the daily nitrogen by soda-lime was 255·19 grains in the water-, and 253·35 grains in the wine-period.

Nitrogen after claret.

Days.	Urea.	Nitrogen calculated from urea.	Nitrogen by soda-lime.
	grammes.	grammes.	grammes.
21st day	45·500	21·233	20·779
22nd day	42·900	20·020	
23rd day	38·112	17·780	18·159
24th day	36·960	17·448	17·640
25th day	14·119
26th day	42·900	20·020	
27th day	41·128	21·193	
28th day	44·646	20·805	20·110
29th day	38·739	18·078	18·548
30th day	27·777	12·938	13·324
Means	39·851	18·883	17·525

As one determination of urea and three determinations of nitrogen by soda-lime were lost, in order to find the daily amount of nitrogen in the whole of the ten days, the soda-lime nitrogen of the 25th day may be added to the total ureal nitrogen, and the mean taken. If this be done, the mean daily excretion of nitrogen was 18·362 grammes. This gives an excess of no less than 1·682 gramme over the first period, and 1·504 over the wine-period. The excess was so large, and was so unlike anything seen before during any of the experiments, as to prove it was not accidental.

The question now arises, if the increase was owing to the direct effect of the wine. This seems unlikely, partly because some evidence of increase would then have been obtained from the ten days during which the wine was taken, and partly from another reason. During this last period the man became ill; he was not feverish, but his pulse was quick. On the 25th, 26th, and 27th days there was some looseness of the bowels and headache; he could scarcely eat his food, and lost weight for the first time.

On the 29th day he was better, and on the 30th felt quite well, and on that day the nitrogen (as determined in both ways) fell greatly. He ascribed his illness to the monotony of his life, the sameness of his diet, and the comparative want of exercise, whilst it is also possible that the

wine, to which he was unaccustomed, and the small allowance of water, may have had some effect in deranging his nutrition. It seems, however, fair to conclude that the wine had only an indirect share, if any, in causing this illness and increased elimination, which was manifestly caused by some peculiar morbid state of nutrition. It is noticeable in this case that there was increased elimination of nitrogen (evidently in the form of urea), without any increase in the mean temperature of the body. There was, however, an increase in the temperature at 2, 4, and 6 o'clock, when digestion was most active. The gradual loss of weight of the body was very striking.

The phosphoric acid, chlorine, and free acidity in the urine.

Days.	Phosphoric acid.	Chlorine.	Free acidity calculated as crystallized oxalic acid.
	grammes.	grammes.	grammes.
1st day	1.390	7.295	
2nd day	2.001	8.571	
3rd day	2.338	9.045	
4th day	2.348	9.242	1.865
5th day	2.327	7.011	1.415
6th day	2.460	8.307	2.192
7th day	2.328	6.174	1.890
8th day	2.582	6.688	1.891
9th day	2.340	7.799	2.538
10th day	2.272	7.072	1.892
11th day	2.132	6.487	1.828
12th day	2.234	6.399	1.885
13th day	2.352	5.524	1.756
14th day	2.450	6.658	1.940
15th day	2.333	5.403	1.486
16th day	2.442	5.793	2.709
17th day	2.577	5.999	2.972
18th day	2.132	6.498	2.211
19th day	1.942	7.045	2.182
20th day	1.881	6.235	2.000
21st day	2.678	6.276	2.784
22nd day	2.405	7.422	2.457
23rd day	2.265	6.543	2.782
24th day	2.453	7.494	2.469
25th day	2.138	7.713	2.013
26th day	2.286	10.763	1.968
27th day	2.798	7.074	3.339
28th day	3.040	7.025	2.745
29th day	2.722	6.170	2.201
30th day	1.182	5.286	1.489

The mean quantities are as follows :—

	Phosphoric acid.	Chlorine.	Free acidity.
First period (before wine).....	2.296	7.708	1.955
Second period (during wine)	2.247	6.202	2.221
Third period (after wine).....	2.396	7.176	2.435

Red Bordeaux wine, in quantities of 10 and 20 ounces per diem, did not

affect the excretion of phosphoric acid. The effect on the chlorine is uncertain, as that ingredient has such a wide range of variation. It is, however, interesting to note that the mean daily excretion of the whole thirty days is almost precisely the same as the mean daily excretion of the twenty-five days in the previous series (viz. 7.028 grammes as against 6.915 grammes), and this proves the equality of the diet.

The acidity of the urine was increased during the wine-period, and this continued afterwards. It may be observed that the mean free acidity of the former experiments was almost precisely the same as in these experiments during the water-period (viz. 1.974 as against 1.955 gramme), and was very nearly the same in the alcoholic as in the wine-period (viz. 2.342 as against 2.221).

It seems fair to conclude that the free acidity was really increased, and that the increase continued subsequently.

5. THE ALVINE DISCHARGES.

Weight of Stools.

Days.	Ounces.	Grammes.	Days.	Ounces.	Grammes.	Days.	Ounces.	Grammes.
1.	4.5		11.	4.25		21.	3.3	
2.	6		12.		22.		
3.		13.	5.75		23.	■ ■ ■	
4.	7.25		14.	8.1		24.		
5.	4		15.	4		25.	6.75	
6.	4.25		16.	4		26.	4.5	
7.	3.75		17.		27.	4.75	
8.		18.	7.25		28.	3.25	
9.	3.75		19.	8.75		29.		
10.	7.97		20.	3.5		30.	7	
Means	4.147	117.56	4.000	115.1	3.788	107.4

The nitrogen was determined twice, viz. on the 10th day (last day before wine), and on the 19th day (last day but one of wine). Unfortunately there had been some constipation before the 10th day, and the stool was unusually copious and less watery; it represented, in fact, some accumulation, and therefore the nitrogen ought to be credited in part to the previous days.

The following Table gives the results:—

Days.	Weight of stool.		Percentage.			Amount of nitrogen in 24 hours.
			Solids.	Water.	Nitrogen.	
10th day (water drinking)	ounces.	grammes.				grammes.
19th day (wine drinking)	7.97	223.0	32.405	67.595	1.204	2.925
	3.75	106.3	21.820	78.180	1.207	1.263

Looking to the mean weight of all the stools, to the particular circumstances of the 10th day's stool, and the very nearly equal percentage of nitrogen on the 10th and 19th days, it may be concluded that the wine did not affect the intestinal discharges either as regards quantity or nitrogen.

6. THE ELIMINATION OF ALCOHOL.

As in the former series, the numerous experiments we had to perform prevented us from thoroughly investigating this difficult problem. We tested the appearance of alcohol in the excreta by the bichromate-of-potassium test as before. The general results were as follows :—

Elimination by the breath.

In the first period the bichromate test was not tried on the first day ; it was very slightly changed in colour on the 2nd, 3rd, and 4th days, when the breath was blown through the test for 15 minutes about 2 o'clock. On the remaining 5th, 6th, 7th, 8th, 9th, and 10th days, no change was produced. On the 1st day of wine after dinner, the colour became green in eight minutes, on the 2nd day in six minutes, and subsequently a little sooner. On the 16th and subsequent days (when the wine was doubled) the change was much greater. In the evening, except in one or two cases, no change was produced. On the 21st day (1st day after wine) and subsequent days there was no alteration.

The breath was condensed by a freezing-mixture on the 9th day about 4 o'clock ; about $\frac{1}{2}$ cub. centim. was collected ; it was tested for alcohol by the Iodoform test, but none was found ; it was unfortunately not examined by the bichromate test. On the 20th day (20 ounces of wine) the breath was again condensed ; it gave an immediate marked green reaction with the bichromate test. On the 22nd day (the 2nd after the wine) it was again condensed, and gave still an immediate reaction, though not so marked as on the 20th day ; so that two days after the wine was left off, some was passing off by the lungs, though it was not detected by merely breathing through the test.

On the 25th and 28th days, when the breath was again condensed, no effect was produced on the bichromate test.

Elimination by the skin.

In the former series of experiments, when the perspiration was obtained by putting the arm in an hermetically sealed glass jar, no effect was produced in the bichromate test by the sweat before alcohol had been taken.

But on this occasion, when 12 cub. centims. of perspiration were collected in four hours on the 5th day, the bichromate test was at once made green. No alcohol was detected by the Iodoform test, but we are not certain if this can be relied upon. This was on the 17th May, and no alcoholic liquid had been taken since the 25th April.

It seemed improbable that alcohol, taken so long before, could be still passing off; and if not, then the perspiration may at times contain some non-alcoholic substance capable of reducing the bichromate.

The perspiration of the arm was condensed on the 10th day (before wine), on the 19th day (during wine), and on the 26th, 28th, and 30th days (after wine). In all cases an extremely marked green reaction was at once given.

We conclude, therefore, that fresh experiments are necessary with regard to the correctness of the bichromate test, when applied to the condensed perspiration.

Elimination by the kidneys.

The examination was conducted in the same way as on the former occasion, the urine being first distilled, the distillate tested with the bichromate test, and if no reaction was given redistilled.

The following Table gives the results:—

Days.	Reaction with bichromate.	
	1st distillate.	2nd distillate.
6th day (water).....	A very slight and scarcely perceptible change.	A very slight change, scarcely to be affirmed*.
15th day (wine, 10 oz.) ...	No change.	No change*.
16th day (wine, 20 oz.) ...	No change.	No change*.
18th day (wine, 20 oz.) ...	No change.	Slight.
20th day (wine, 20 oz.) ...	Slight.	Marked.
22nd day (water)	None.	None.
27th day (water)	None.	None.

We conclude from this Table that when 10 ounces of wine (containing 1·1 ounce of absolute alcohol) were taken, no alcohol passed into the urine. On the 16th day, when 20 ounces (=2·2 ounces of absolute alcohol) were taken, none was found in the urine; the next day no examination was made, but on the 18th day alcohol was detected, and two days later the reaction was marked. Two days after the wine was left off no alcohol was found.

Therefore, when this man took 2 ounces of absolute alcohol day after day, some of it was eliminated by the urine. When he took only 1 ounce, none was eliminated during the space of five days. If, as has been surmised by Dr. Anstie, the appearance of alcohol in the urine indicates that there is an excess in the body, it seems clear that this man cannot take much more than 1 ounce without the urine giving evidence of it, and thereby proving excess. It soon disappeared from the urine, certainly on the 2nd day (the first day's urine was not examined), whereas, on the former occasion, when a much larger quantity had been taken, it could be detected five days after it had been discontinued.

* Tested also with the Iodoform test. No reaction.

Elimination by the bowels.

No experiments were made.

GENERAL CONCLUSIONS.

1. The general results of these experiments are in all respects identical with the experiments on alcohol and brandy, that is to say, there was a marked effect on the heart, coinciding tolerably well in amount with the effect produced by pure alcohol in the former experiments; there was no unequivocal alteration of temperature in the axilla or rectum, no alteration in the elimination of nitrogen, for the increase in the last period cannot be credited to the direct effect of the wine; no alteration in the phosphoric acid of the urine; some augmentation of the free acidity of the urine; no alteration of the alvine discharges. In other words, claret-wine in the above quantities cannot so far be distinguished in its effect from pure alcohol. Its most marked effect, the increase of the heart's action, must be ascribed to the alcohol, in great measure, though the ethers may play some slight part.

But it would be going too far to assert that the dietetic effects of red Bordeaux wine and of dilute alcohol are identical. The difference between them must probably be sought in their effects on primary digestion and assimilation, delicate and subtle influences which experiments like those recorded in the paper do not touch. The influence of the sugar, of the salts, and of the acidity must also be appreciated by other methods. The man himself affirmed that the wine agreed with him better than the alcohol or brandy, but the large quantity he took of these last fluids vitiates the comparison.

These experiments on wine enabled us to define somewhat better than the previous trials what might be considered moderation for this man. The 10 ounces of wine, containing about 1 fluid ounce of pure alcohol, did not cause the least unpleasant feeling of heat or flushing. The 20 ounces (containing almost 2 fluid ounces of alcohol) were manifestly too much. He felt hot and uncomfortable, was flushed, the face was somewhat congested, and he was a little drowsy. Moreover, as already mentioned, alcohol then began to appear in the urine. Therefore he ought certainly not to take much more than 1 fluid ounce of absolute alcohol in 24 hours.

With regard to the propriety of this healthy man taking any alcohol, we have no hesitation in saying he would be better without it. His heart naturally acts quickly and strongly enough; alcohol increases its action too much, and might lead on to alteration in its condition, or to injury of vessels, if any degeneration were to take place in them. This man had gone through the Abyssinian campaign, and stated that when the force was without rum, owing to deficiency of transport beyond Antalo, he had in no way felt the want of the stimulant, though some of his comrades did. This seems to confirm our opinion, that alcohol for him is not a necessity, and indeed is not desirable.

"On the Mathematical Theory of Combined Streams." By W. J. MACQUORN RANKINE, C.E., LL.D., F.R.SS. Lond. and Edin.
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1. *Object of this Investigation.*—The principles of the action of combined streams were to a certain extent investigated by Venturi, and stated in his essay 'Sur la Communication latérale du Mouvement dans les Fluides' (Paris, 1798). The principle of the conservation of momentum, so far as I know, was first explicitly applied to combined streams by Mr. William Froude, F.R.S., in a paper on Giffard's Injector, read to the British Association at Oxford, in 1860, and published in the Transactions of the Sections, p. 211. Various other authors have treated the same problem by different methods, based virtually on the same principle. A very complete and precise investigation of the theory of combined streams, in every case in which two streams only are combined, is contained in Professor Zeuner's treatise 'Das Locomotivenblasrohr' (Zürich, 1863). The theoretical conclusions are tested by comparison with experiment, and applied to practical questions, especially those relating to the apparatus from which the treatise takes its name. The object of the present investigation is to apply similar principles to the combination of any number of streams; and the demonstration of the fundamental dynamic equation differs from that given by Zeuner in method, though not in principle, being effected at one operation by the direct application of the principle of the equality of impulse and momentum, instead of by the consideration of the loss of energy that takes place during the combination of the streams.

2. *Terms and Notation used, and Suppositions made.*—The several streams which are combined will be called before their junction, the *component streams*; the stream formed by their combination will be called the *resultant stream*. The passages through which the component and resultant streams flow will be called respectively the *supply-tubes* and the *discharge-tube*. The combination of the streams will be supposed to take place in a short cylindrical chamber, with its axis parallel to the direction of flow, which will be called the *junction-chamber*.

At one end of the junction-chamber are the outlets of the supply-tubes, which will be called the *nozzles*; at the other end, the inlet of the discharge-tube, which will be called the *throat*. It will be supposed, further, that the supply-tubes are so formed as to direct the component streams at the nozzles, so that they shall all flow sensibly parallel to each other and to the resultant stream. The principal symbols used are as follows: for any one of the component streams:—

a , area of nozzle;

v , velocity of flow at nozzle;

s , bulkiness, or reciprocal of density at nozzle.

The several component streams may be distinguished from each other, when required, by suffixes; as 1, 2, 3, &c.

For the resultant stream :

A , area of throat ;

V , velocity of flow at throat ;

S_0 , bulkiness, or reciprocal of density at throat.

Intensities of pressure, in *absolute units* on the unit of area :

p_0 , at the nozzle end of junction-chamber ;

P_0 , at the throat.

(These may be converted into *units of weight* on the unit of area, by dividing by g).

The flow of each stream is supposed to be steady. The fluids may be either liquid, vaporous, gaseous, or mixed.

3. *Equation of Continuity.*—The mass of fluid that enters the junction-chamber through a given nozzle in a unit of time is $\frac{av}{s_0}$. The mass discharged in the same time at the throat is $\frac{AV}{S_0}$. The flow being steady, the following equation must at every instant be fulfilled :

$$\frac{AV}{S_0} = \Sigma \cdot \frac{av}{s_0} \quad \dots \dots \dots (1)$$

If S_0 and the several values of s_0 are given, that equation gives the velocity of the resultant stream in terms of those of the component streams ; viz.

$$V = \frac{S_0}{A} \cdot \Sigma \cdot \frac{av}{s_0} \quad \dots \dots \dots (1A)$$

If all the fluids are liquids, each of sensibly invariable bulkiness, we have also $AV = \Sigma \cdot av$; that is, the volume of flow of the resultant stream is equal to the aggregate of the volumes of flow of the component streams ; but if any or all of the streams are vaporous or gaseous, the values of s_0 will depend upon that of p_0 , and the value of S_0 upon that of P_0 , and upon the changes of bulkiness of the fluids which may take place in the junction-chamber, through change of temperature, change of condition, or chemical action.

In any case S_0 may be regarded as a given function of P_0 , and of the mutual proportions of the several values of $\frac{av}{s_0}$; in other words, of the ingredients in the resultant stream.

4. *Dynamical Equation.*—The aggregate momentum of the mass of fluid that enters the junction-chamber through the nozzles in a unit of time is $\Sigma \cdot \frac{av^2}{s_0}$. The momentum of the equal mass which leaves the junction-chamber through the throat in the same time is $\frac{AV^2}{S_0}$.

The forward impulse exerted in a unit of time upon the mass of fluid in the junction-chamber by the pressure at the nozzle end of the chamber is $p_0 A$. The backward impulse exerted in the same time on the same mass by the pressure at the throat-end of the chamber is $P_0 A$. By the

second law of motion, the difference between those impulses is equal to the change of momentum produced; that is to say,

$$A(P_0 - p_0) = \Sigma \frac{av^2}{s_0} - \frac{AV^2}{S_0} = \Sigma \left\{ \frac{av}{s_0}(v - V) \right\}; \dots (2)$$

or dividing both sides by A ,

$$P_0 - p_0 = \Sigma \frac{av^2}{As_0} - \frac{V^2}{S_0} = \Sigma \left\{ \frac{av}{As_0}(v - V) \right\}. \dots (2A)$$

And this is the general dynamical equation of the combination of any number of streams of any fluids.

If the preceding equation, as applied to a combination of two streams only, be compared with the equation not numbered, which immediately precedes equation 60 in Zeuner's treatise, it will be seen that they are virtually identical, although different in form, and demonstrated by different methods.

5. *Loss of Energy at Junction.*—If a given mass of any fluid at the bulkiness s and pressure p is contained in a reservoir, from which it is capable of being expelled by the inward motion of a piston loaded with an external force equivalent to the pressure, it is known that the potential energy of the mass of fluid and of the piston relatively to a point at the level of the centre of mass of the fluid is expressed by multiplying the mass by $\int_0^p s dp$, the relation between s and p being that which is called *adiabatic*; that is to say, such that no heat is received or given out by the fluid. Hence the loss of energy in the junction-chamber in each unit of time is given by the following expression:—

$$\Sigma \left\{ \frac{av}{s_0} \left(\frac{v^2}{2} + \int_0^{p_0} s dp \right) \right\} - \frac{AV}{S_0} \left(\frac{V^2}{2} + \int_0^{P_0} S dP \right), \dots (3)$$

of which the first, or positive term, denotes the aggregate energy, actual and potential, of the component streams as they enter the junction-chamber; and the second, or negative term, expresses the total energy, actual and potential, of the resultant stream as it leaves that chamber. That lost energy takes the form partly of visible eddies and partly of invisible molecular motions—that is, of heat.

The integral expressing the aggregate potential energy of the component streams may be put in the following form:—

$$\int_0^{p_0} \left(\Sigma \frac{avs}{s_0} \right) dp. \dots (3A)$$

If no change of total bulkiness arises from the mixture of the component streams, the volume occupied by a given mass of the mixture is simply the sum of the volumes of its ingredients; so that we have

$$\frac{AVS}{S_0} = \Sigma \frac{avs}{s_0}; \dots (3B)$$

and the expression for the loss of energy becomes

$$\Sigma \cdot \frac{av^2}{2s_0} - \frac{AV^2}{2S_0} - \frac{AV}{S_0} \int_{p_0}^{P_0} S dP. \quad (3C)$$

When the fluids are all liquids, whose compressibility may be neglected, we have $\int_{p_0}^{P_0} S dP = S_0 (P_0 - p_0)$; and substituting for the difference of pressures its value, according to equation (2), the following expression is found for the loss of energy at the junction,

$$\Sigma \cdot \left\{ \frac{av}{s_0} \cdot \frac{(v-V)^2}{2} \right\}. \quad (3D)$$

that is to say, in the case of liquids all the energy due to the several velocities $(v-V)$ of the component streams relatively to the *resultant stream* is lost.

When the expression (3D) is reduced to a single term, it becomes the well-known value of the loss of energy of a single stream of liquid at a sudden enlargement in a tube.

6. *Efficiency of Combined Streams.*—The *efficiency* of a set of combined streams may be defined as the fraction expressing the ratio borne by the total energy of the resultant stream after the combination to the aggregate energy of the component streams before the combination. It is expressed as follows:—

$$\frac{\frac{AV}{S_0} \left\{ \frac{V^2}{2} + \int_0^{P_0} S dP \right\}}{\Sigma \left\{ \frac{av}{s_0} \left(\frac{v^2}{2} + \int_0^{P_0} s dp \right) \right\}}. \quad (4)$$

7. *General Problem of Combined Streams.*—In most cases the problem of combined streams takes one or other of the two following forms. In each of the two forms the areas of the nozzles $a_1, a_2, \&c.$ are given, and also the area of the throat, A .

First Form.—The quantities given, besides the before-mentioned areas, are the pressure at the nozzles, p_0 , and the velocities of the component streams, $v_1, \&c.$ The functional values given are those of $s_{0,1}, s_{0,2}, \&c.$, in terms of p_0 , and of S_0 in terms of $P_0, \frac{a_1 v_1}{s_{0,1}}, \frac{a_2 v_2}{s_{0,2}}, \&c.$ Those functional values are to be substituted in the equations (1) and (2); and the solution of these equations will give the numerical values of V and of P_0 . In the case of liquids of sensibly constant bulkiness, $s_{0,1}, \&c.$, and S_0 are quantities sensibly independent of p_0 and P_0 ; and then equations (1) and (2) can be separately solved without elimination, giving respectively V and P_0 .

Second Form.—Each of the component streams flows through a passage whose factor of resistance, f , is given, from a separate reservoir in which the pressure p and the elevation z of the surface above the junction-chamber are given. The resultant stream flows through a passage whose

factor of resistance, F , is given, into a reservoir in which the pressure P and the elevation Z of the surface above the junction-chamber are given. These, together with the areas A , a_1 , a_2 , &c., are the quantities given. The functional values given are those of the bulkiness, $s_{0,1}$, $s_{0,2}$, &c., and S_0 , as before; also the following values of the velocities, according to well-known principles in hydrodynamics; for any component stream,

$$v = \sqrt{\left\{ \frac{2gz + 2 \int_{p_0}^P s dp}{1 + f} \right\}}; \quad \dots \dots \dots (5)$$

and for the resultant stream,

$$V = \sqrt{\left\{ \frac{2gZ + 2 \int_{P_0}^P S dP}{1 + F} \right\}}; \quad \dots \dots \dots (6)$$

The functional values given are to be substituted in equations (1) and (2), whose solution will then give the numerical values of p_0 and P_0 ; and from these and the other data the numerical values of v , &c. and of V may be calculated.

November 17, 1870.

General Sir EDWARD SABINE, K.C.B., President, in the Chair.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair.

General Boileau, Mr. Busk, Mr. David Forbes, Sir John Lubbock, and Mr. Mivart, having been nominated by the President, were elected by ballot Auditors of the Treasurer's accounts on the part of the Society.

Mr. Andrew Noble, Capt. Sherard Osborn, and Mr. George Frederic Verdon were admitted into the Society.

Anders Jöns Ångström, of Upsala, and Joseph Antoine Ferdinand Plateau, of Ghent, were proposed for election as Foreign Members, and notice was given from the Chair that these gentlemen would be ballotted for at the next Meeting.

The Presents received were laid on the table, and thanks ordered for them.

The following communications were read:—

- I. "Researches into the Chemical Constitution of the Opium Bases.—Part IV. On the Action of Chloride of Zinc on Codeia." By AUGUSTUS MATTHIESSEN, F.R.S., Lecturer on Chemistry at St. Bartholomew's Hospital, and W. BURNSIDE, of Christ's Hospital. Received June 23, 1870. (See page 71.)

- II. "Experiments on the Action of Red Bordeaux Wine (Claret) on the Human Body." By E. A. PARKES, M.D., F.R.S., Professor of Hygiene in the Army Medical School, and Count CYPRIAN WOLLOWICZ, M.D., Assistant Surgeon, Army Medical Staff. Received July 5, 1870. (See page 73.)
- III. "On the Mathematical Theory of Combined Streams." By W. J. MACQUORN RANKINE, C.E., LL.D., F.R.SS. Lond. and Edinb. Received Sept. 10, 1870. (See page 90.)
- IV. "On the Fossil Mammals of Australia.—Part IV. Dentition and Mandible of *Thylacoleo Carnifex*, with Remarks on the Argument for its Herbivory." By Prof. OWEN, F.R.S. &c. Received September 27, 1870.

(Abstract.)

In this paper the author, referring in the Introductory Section (§ 1) to objections published to his former restorations and inferences as to the function of the dentition of *Thylacoleo*, proceeds to give descriptions, with figures, of (§ 2) an upper jaw and maxillary teeth, and (§ 3) of a portion of the mandible with mandibular teeth, from tertiary deposits at Gowrie Creek, Queensland, presented to the British Museum by Sir Daniel Cooper, Bart.

He then describes certain specimens and photographs of maxillary teeth (§ 4), and of mandibular teeth (§ 5) of the *Thylacoleo*, subsequently obtained by Prof. A. M. Thomson, of Sydney, and Gerard Krefft, Esq., Curator of the Museum of Natural History, Sydney, New South Wales, from caves in Wellington valley, for the exploration of which a grant had been voted by the Local Legislature of New South Wales.

Section 6 is given to a description of the specimen in the British Museum, and a cast in the Museum at Sydney of an entire inferior incisor, transmitted, with the photograph above mentioned, to the author. The guiding principle in inferring function from form of teeth is next defined (§ 7), and the author proceeds to discuss the objection from the location of lanianaries in § 8. The dentitions of *Thylacoleo* and of *Phascolarctos* are compared in § 9; and the results contrasted with those of the advocates of the herbivory of both genera, which were illustrated by the figures 2 & 4 in the 'Quarterly Journal of the Geological Society,' vol. xxiv. pp. 312, 313 (1868).

In § 10 the deductions from the mandibular characters of carnivorous and herbivorous marsupials are tested, and those characters illustrated by descriptions and figures of the lower jaw in *Thylacoleo*, *Cheiromys*, *Plagiulax*, *Thylacinus*, *Sarcophilus*, *Phascolarctos*, and *Hypsiprymnus*. The testimony to the native food of the Aye-aye is sifted in § 11, and the

bearing of the characters of its mandible and dentition on the question of the carnivory or herbivory of *Thylacoleo* is weighed. A like comparison and physiological consideration are applied to the mandibular characters of *Thylacoleo*, *Plagiaulax*, and the true Rodentia in § 12; and the author next (§ 13) proceeds to the consideration of the form, structure, and growth of the large incisors in the Diprodont paucidentate Marsupials, and in the lemurine and lissencephalous Rodents. To the affirmation of "the obviously phytophagous type of the incisors of *Thylacoleo* and *Plagiaulax*," the author, referring to the descriptions and figures of those teeth in the preceding part of his paper, enters upon a consideration of the relations of their differences from those teeth in the truly phytophagous Marsupials and Placentals to interrupted and continuous application of teeth (§ 14). The alleged adaptability of the carnassials in *Thylacoleo* to reduction of vegetable food leads next to a consideration of the work of the molar machinery in known existing *Herbivora* (§ 15). In section 16 the place, and especially the family relations, of the *Thylacoleo* in the Marsupial order are considered. Instances of existing diprotodonts subsisting on animal food, and of existing polyprotodonts on vegetable food, are adduced; and, after comparisons with the genera *Macropus*, *Halmaturus*, *Lagorchestes*, *Heteropus*, *Petrogale*, *Osphranter*, *Dendrolagus*, *Hypsiprymnus*, *Bettongia*, *Potorous*, *Dorcopsis*, *Cuscus*, *Phascolarctos*, *Phalangiata*, *Hepoona*, *Dactylopsila*, *Petaurus*, *Belideus*, *Acrobata*, *Petaurista*, *Dromicia*, *Tarsipes*, the author is led to assign *Plagiaulax* and *Thylacoleo* to a distinct family of Diprotodont Marsupials under the name "*Paucidentata*," in reference to the reduction of the molar teeth to one on each side of the upper jaw, and two on each side of the lower jaw. He then (in § 17) discusses the reality and value of the indications of tendency from the "general to the particular" in the dentition of the mesozoic and neozoic Paucidentate Marsupials. The objections to the predaceous nature of *Thylacoleo* and *Plagiaulax* from their alleged feebleness and dwarfishness are discussed in § 18. The grounds on which John Hunter was led to refer the molar of *Mastodon ohioiticus* to either a carnivorous or a mixed-feeding animal, and those on which the author refers the dentition and skull of *Thylacoleo* to a carnivorous species, are contrasted, and the nature of a disparaging comparison is exposed in § 19.

The author concludes by a description of certain unequal phalanges, which supported a strong claw, bound close by a basal bony sheath, as in the Lion, obtained from the breccia-caves of Wellington valley, and for which, among the fossils thence exhumed, there is not, at present, any other claimant save *Thylacoleo*.

November 24, 1870.

General Sir EDWARD SABINE, K.C.B., President, in the Chair.

In pursuance of the Statutes, notice was given from the Chair of the ensuing Anniversary Meeting, and the list of Officers and Council proposed for election was read as follows :—

President.—General Sir Edward Sabine, R.A., K.C.B., D.C.L., LL.D.

Treasurer.—William Spottiswoode, Esq., M.A.

Secretaries.— { William Sharpey, M.D., LL.D.
George Gabriel Stokes, Esq., M.A., D.C.L., LL.D.

Foreign Secretary.—Prof. William Hallowes Miller, M.A., LL.D.

Other Members of the Council.—George Burrows, M.D.; Heinrich Debus, Esq., Ph.D.; Prof. Peter Martin Duncan, M.B.; Sir Philip de M. Grey-Egerton, Bart.; Prof. George Carey Foster, B.A.; Francis Galton, Esq.; John Peter Gassiot, Esq., D.C.L.; Joseph Dalton Hooker, C.B., M.D.; William Huggins, Esq., D.C.L., LL.D.; Prof. George M. Humphry, M.D.; John Gwyn Jeffreys, Esq.; Sir John Lubbock, Bart.; Charles William Siemens, Esq., D.C.L.; Prof. Henry J. Stephen Smith, M.A.; Prof. John Tyndall, LL.D.; Prof. Alexander W. Williamson, Ph.D.

Pursuant to notice given at the last Meeting, Sir John Rennie proposed and General Boileau seconded His Grace the Duke of Sutherland for election and immediate ballot.

The ballot having been taken, the Duke of Sutherland was declared duly elected.

Pursuant to notice given at the last Meeting, Anders Jöns Ångström, of Upsala, and Joseph Antoine Ferdinand Plateau, of Ghent, were balloted for and elected Foreign Members of the Society.

The following communications were read :—

- I. "Communication from the Secretary of State for India relative to Pendulum Observations now in progress in India in connexion with the Great Trigonometrical Survey under the Superintendence of Colonel J. T. WALKER, R.E., F.R.S." Read by order of the President and Council.

India Office, S.W., 3rd October, 1870.

SIR,—I am directed by the Secretary of State for India to transmit to you, for the information of the President and Council of the Royal Society, the enclosed copy of a letter from Colonel Walker, the Superintendent of the Great Trigonometrical Survey of India, on the pendulum-observations that have been carried on since 1865 by Captain Basevi, together with a note, tabulated results, and a Map of India showing the pendulum-stations.

The Duke of Argyll will be obliged if, in accordance with Colonel Walker's wish, the President and Council would be so good as to furnish

His Grace with any suggestions that may occur to them with reference to supplementary measures that may appear necessary in order to complete the operations which were commenced at the suggestion of General Sabine, and with the concurrence of the Council.

I am, Sir,

Your obedient Servant,

J. COSMO MELVILL.

The Secretary to the Royal Society.

Enclosure No. 1.

Government of India, Home Department—Geographical.

To His Grace the Right Honourable the Duke of Argyll, Kt., Her Majesty's Secretary of State for India.

Simla, the 26th of August, 1870.

MY LORD DUKE,—Referring to Sir Charles Wood's despatch in the Military Department No. 271, dated the 23rd of August, 1864, authorizing the carrying out of certain pendulum experiments in connexion with the operations of the Great Trigonometrical Survey of India, at the recommendation of the President and Council of the Royal Society, we have the honour to transmit, for your Grace's information, copy of a letter from Colonel Walker, No. 49-793, dated the 11th instant, together with its enclosures, showing what has been done and what remains to be done to complete the original programme.

2. With reference to the last paragraph of Colonel Walker's letter, we beg that the President and Council of the Royal Society may be invited to suggest, at an early date, any supplementary measures which they may consider desirable.

We have the honour to be,

My Lord Duke,

Your Grace's most obedient, humble Servants,

MAYO.

NAFIER OF MAGDALA.

JOHN STRACHEY.

R. TEMPLE.

J. F. STEPHEN.

B. H. ELLIS.

H. W. NORMAN.

Enclosure No. 2.

From Colonel J. T. Walker, R.E., Superintendent Great Trigonometrical Survey of India, to the Secretary to the Government of India, Home Department, Simla.

Dated Mussoorie, 11th August, 1870.

SIR,—I have the honour to report that the pendulum-observations which have been carried on since the year 1665, by Captain Basevi, in con-

nexion with the operations of the Trigonometrical Survey of India, at the recommendation of the President and Council of the Royal Society, are now nearly completed, in conformity with the original programme of operations which was sanctioned by the Right Honourable the Secretary of State for India, in his military letter No. 271, dated 23rd August 1864, to the Governor-General in Council.

(2.) The results are of much importance, not only as affording independent information on the figure of the earth, but as throwing some light on "the laws of the local variations of gravity which are superposed on the grand variation from the poles to the equator;" thus it will, I trust, be conceded that they amply fulfil the purposes contemplated in the 'Correspondence and Proceedings of the Council of the Royal Society concerning Pendulum-Observations in India.'

(3.) But, before the operations are brought to a close, I think it is desirable that the President and Council of the Royal Society should be informed of what has been done hitherto, and of what remains to be done to carry out the original programme of operations; also that they should be invited to suggest any supplementary measures which they may consider necessary in order to complete the operations, and thus perfect a work which was commenced at the suggestion of the President and with the hearty approval of the Council, and in the success of which they take a lively interest.

(4.) I have therefore prepared the accompanying note on the operations in explanation of what has been done hitherto, and of what remains to be done to complete the original programme; and I beg leave to request that the Secretary of State may be moved to communicate it to the President and Council of the Royal Society, and to invite their opinions and suggestions. The Note is accompanied with a map on which the positions of the pendulum stations are indicated.

I have the honour to be, Sir,

Your most obedient Servant,

J. T. WALKER, Colonel R.E.,

Supdt. Great Trigonometrical Survey of India.

Note on the Pendulum-observations in India, which are being carried on by Captain J. P. Basevi, in connexion with the operations of the Great Trigonometrical Survey of India.

The observations have been made with the two invariable pendulums of the Royal Society, which are known as No. 4 and No. 1821. The number of vibrations in twenty-four hours is determined by observing the coincidences of each pendulum with the pendulum of a clock by Shelton, which is also the property of the Royal Society. The pendulums are swung, one at a time, in the receiver of a vacuum apparatus out of which as much air as possible is withdrawn by an air-pump, and the rate of the clock is determined every night.

(2.) Captain Basevi's daily course of procedure is as follows. At 6 A.M. he sets in motion the pendulum which is under observation. At 7 A.M. he observes three coincidences and reads the thermometers and pressure-gauge. Between 7 A.M. and 4 P.M. he observes a coincidence and reads the thermometers and the gauge, five times at intervals of $1\frac{1}{4}$ hour. At 4 P.M. he closes this portion of the work by observing three coincidences and again reading the thermometers and the gauge. Thus for nearly ten hours of the day Captain Basevi never permits himself to be absent for more than a few minutes at a time from the pendulums. These frequent observations are necessary in order that the temperatures may be exactly determined. At 8 to 10 P.M. he observes transits.

(3.) Originally it was expected that, by employing a vacuum-apparatus, the pendulum might be vibrated for twenty-four hours before the vibrations became too small for the observation of coincidences, and consequently that the rate derived from the coincidences would be wholly independent of irregularities in the clock's rate in different parts of the twenty-four hours. But this would have necessitated observations of the temperature at regular intervals throughout the twenty-four hours, which, as a rule, would have been impossible, though a few such groups of observations have been taken experimentally. Moreover at the commencement of the operations the vacuum-cylinder could not be made sufficiently air-tight to admit of so protracted an observation.

(4.) Each pendulum is observed a certain number of days with the face to the front, and then as many days with the face to the rear. At the first four stations observations were taken for five days on each face, making altogether twenty days' observations for both pendulums; as, however, it was found that the theoretical probable error of the mean of the ten days' observations by a single pendulum was only $\pm \cdot 05$ of a vibration, the number of observations was subsequently limited to six days on both faces, making altogether twelve days' work at each station.

(5.) The observations are now being printed in the office of the Trigonometrical Survey, and a few specimen pages accompany this note. A preliminary abstract of the mean results by both pendulums is also given, and a map indicating the positions of the stations of observation*.

(6.) The results obtained hitherto are not final; the coefficients of the corrections for temperature and pressure have not yet been conclusively determined, and the reductions to mean sea-level will probably be effected when the calculations of the influence of local irregularities in the crust of the earth have been carried to a greater distance from the stations than has hitherto been practicable.

(7.) Of these corrections the most important is that for temperature; the mean temperature of the observations ascends from a minimum of 54° at the base station Kew, to a maximum of 88° at Namthabad, being a

* [It has not been thought requisite to publish this map.—G. G. S.]

range of 34° ; as the correction is approximately equal to one vibration for 2° of temperature, or seventeen vibrations for the extreme range, the true value must necessarily be determined with the utmost possible accuracy.

(8.) In Section XIII. of my General Report on the Operations of the Trigonometrical Survey for 1866-67, I have fully described certain measures which were taken to determine the coefficient of linear expansion. Briefly, they were as follows: vibrations were observed, at high and low temperatures, under the lowest pressure which could be obtained in the vacuum-apparatus at Kaliana, and at the natural pressure at Masoori; the expansions were also determined at high and low temperatures by direct micrometric measurement, with the following results:—

	Pressure, in inches.	Factor of expansion for 1° Fahrenheit.	
At Kaliana ..	3.5	000,011,10	} by vibrations.
„ Masoori ..	23.5	000,010,01	
„ Dehra	27.7	000,009,73	by direct measurement.

Thus the value of the expansion which was determined from vibrations under a pressure of 3.5 inches was 14 per cent. greater than the value determined by direct measurement, at the natural pressure. I stated in my report that “whether this is due to an actual increase of expansion for a decrease of pressure or to the action of other phenomena which are at present unknown or only imperfectly known, is a problem for future solution.”

(9.) Experiments have been made at the Kew Observatory for the purpose of investigating this question; they are described in the ‘Proceedings of the Royal Society,’ No. 113, 1869. Owing, however, to difficulties which were experienced in working with artificial temperatures, the results were not conclusive as regards the present difficulty, and the hope was expressed that the question would find its best solution by our labours in India.

(10.) The temperature-coefficients which have been employed in the preliminary reductions are those which were obtained from the observations at Kaliana, viz.:

For No. 4 pendulum 0.485 vibration per diem for 1° Fahrenheit.

„ No. 1821 „ 0.470 „ „ „

(11.) The pressure-coefficient which has been employed hitherto is the mean of the two values determined at Kew, or 0.32 vibration per diem for each inch of pressure at 32° Fahrenheit.

(12.) In the reductions to the sea-level, the surface-density has been assumed to be half the mean density of the earth. Dr. Young’s formula has been used exclusively for stations situated on tolerably level plains, but for stations on hills the observations have been first reduced to the general level of the country by computing the vertical attraction of the elevated mass down to this level, the mass being divided into a number of

compartments by concentric circles and radii; they have then been reduced to the sea-level by Dr. Young's formula. The stations thus corrected are Masoori, Usira, Ehmampur, and Somtana; at Masoori, curvature was taken into account, and the calculations were extended to a distance of 100 miles all round; but at the three other stations curvature was not allowed for, as the calculations were only carried to a distance of one mile.

(13.) The preceding details will suffice to explain all that is necessary regarding the observations, and the preliminary results which have been derived therefrom which accompany this note. I will therefore now proceed to indicate the principles by which we have been guided in selecting the positions of the pendulum-stations.

(14.) In the first instance, the original programme of observing at a certain number of stations of the Great Arc was duly carried out; the pendulums were swung at eighteen stations between Cape Comorin and the Siwalik Hills at the base of the southern slopes of the Himalayas, and at two stations north of the Siwaliks.

(15.) As yet no observations have been taken on the higher ranges, or on the tablelands, of the Himalayas, and thus the full influence of these ranges in producing local variations of gravity has not yet been ascertained. But the observations at the five northernmost stations indicate that there is much probability that the density of the strata of the earth's crust under and in the vicinity of the Himalayan mountains is less than that under the plains to the south, the deficiency increasing as the stations approach the Himalayas, and being greatest when they are north of the Siwaliks. On the other hand, the observations at the five southernmost stations show an increase of density in proceeding from the interior of the Peninsula to the coast of Cape Comorin. Thus both groups of observations tend to confirm the hypothesis that there is a diminution of density in the strata of the earth's crust under mountains and continents, and an increase of density under the bed of the ocean.

(16.) In order to test this hypothesis still further, as soon as the observations at the stations of the Meridional Arc were completed, the pendulums were taken to an ocean station—the Island of Minicoy, in the same latitude as Punnæ, and about 250 miles from the mainland; and afterwards to five stations on the east and west coasts, each in nearly the same latitude as one of the stations in the Meridional Arc. Thus the comparisons between the local variations of gravity under the continental, the coast, and the ocean stations are independent of an exact knowledge of the normal variation of gravity in proceeding from the poles to the equator. It will be seen that, without a single exception, gravity at a coast station is in excess of gravity at the corresponding inland station, and that at the ocean station it is greater than at the corresponding coast station; thus:

At Alleppy the pendulum makes 2·41 vibrations more than at Mallapatti.

„ Mangalore	„	2·62	„	Bangalore.
„ Madras	„	2·42	„	Bangalore.
„ Cocanada	„	2·78	„	Kocundal.
„ Calcutta	„	3·19	„	Ehmadpur.
„ Minicoy	„	3·90	„	Punnæ.

(17.) I may observe that the coast stations were selected at places as far removed as possible from mountain-ranges, in order that the results might not be affected by the local variations of gravity under mountains. For this reason additional stations could not be obtained on the west coast, because to the north of Mangalore there is a range of mountains running parallel to the coast at a very short distance.

(18.) Having completed these observations, Captain Basevi returned to the head quarters at Dehra Doon last April, taking a set of observations at Kaliasa *en route*, in order to ascertain whether the times of vibration of the pendulums had sensibly altered, through accident or wear of the knife-edges, in the period of four years which had elapsed since 1866, during which the apparatus had been transported (chiefly by land, but partly by sea) over a distance of several thousand miles, and the pendulum had been swung at twenty-two stations. The result indicates a slight alteration in the pendulums, probably by wear of the knife-edges, to an extent equivalent to one-third of a vibration in twenty-four hours.

(19.) It now remains for Captain Basevi to investigate the true vacuum and temperature corrections, by experiments under artificial temperatures. He is at present making the requisite preliminary arrangements for the purpose, and will commence the experiments as soon as possible. They should be completed by the time that the snows of the approaching winter are sufficiently melted to permit of the passes on the great southern ranges of the Himalayas being crossed. Captain Basevi will then proceed to take observations in the inner Himalayas, on three extensive tablelands which are of great height, and are sufficiently removed from the neighbouring ranges to obviate the necessity for minute calculations of the masses of these ranges, calculations for which the requisite data are not forthcoming. The three tablelands are "the plains of Deosai," lat. $35^{\circ} 5'$, long. $75^{\circ} 30'$, height 13,400 feet; the plains north of the Changchenmo range, lat. $35^{\circ} 15'$, long. $79^{\circ} 20'$, height 16,000 feet; and "the plains of Hanle," lat. $32^{\circ} 50'$, long. 79° , height 14,200 feet. Captain Basevi also proposes to take observations in the plains of the Punjab to the south of the Himalayas. Finally, he will descend the Indus, and take observations on the coast at Karachi, thus obtaining an additional coast station, which will be complementary to an inland station on Colonel Everest's Arc of the Meridian.

(20.) It does not appear necessary that any more observations than these should be taken in India. But in the proceedings of the Council of the Royal Society in which the original programme of observations was dis-

cussed, it was proposed that observations should be taken at points nearer to the equator, at Ceylon, Singapore, or Borneo; also at Aden, a position of interest, "from being in a long line of depression where a large gravitation might be expected." But as one of the two pendulums has already been swung by General Sabine at three stations on or between the equator and the parallel of Punnae, Captain Basevi's southernmost station, and as a pendulum has been swung by Mr. Goldingham at the equator and at the Madras Observatory, which is also one of Captain Basevi's stations, I am inclined to think that there is no immediate necessity for taking observations at Ceylon, Singapore, and Borneo, and that Captain Basevi's operations need not be prolonged for this purpose. On the other hand, however, he will be easily able to observe at Aden; and he might also observe at some point in Egypt, on the plains which are crossed by the Suez Canal, with the great advantage that the stations would be complementary to certain of the stations in India; thus Aden would be compared with Madras and Bangalore, and the plains of Egypt with the Himalayan Mountains.

(21.) I propose, therefore, that Captain Basevi should proceed from Karachi to England, taking observations *en route* at Aden and in Egypt, and bringing his operations to a close by a series of observations at the Greenwich Observatory, if the Astronomer Royal has no objections. I mention the Greenwich rather than the Kew Observatory because the true time can be obtained there from the astronomical clocks, whereas at Kew it can only be obtained by observation; and if (as is probable) Captain Basevi arrives in the winter, pendulum-observations taken at Kew would be greatly delayed, as happened when the operations were commenced at Kew. Moreover, Greenwich appears to have been employed as a reference station for pendulum-observations more frequently than Kew.

J. T. WALKER, Colonel R.E.,
Supt. Great Trigonometrical Survey of India.

Stations.	Geodetic Coordinates.			Mean Temperature.	Mean Pressure in inches.	Observed Number of Vibrations reduced to an infinitely small arc.	Corrections.		Corrected Vibrations at level of Station.	Reduction to Mean Sea-level.	Results.	
	North Latitude.	East Longitude.	Height, in feet.				Reduction to 72° F.	Reduction to a Vacuum.			By Observation.	By computation in terms of Pendulum ellipticity = $\frac{1}{300}$.
Indian Arc Stations.												
1. Pune	8° 10'	77° 41'	44	85.8	1.53	85071.13	+5.61	+0.44	85079.18	+0.11	85079.39	85078.53
2. Kudankolam.	8° 11'	77° 45'	166	86.2	1.46	79.63	0.76	.43	79.01	.43	79.44	79.80
3. Malapatt	9° 20'	78° 3'	343	73.8	1.23	75.91	1.68	.36	77.35	.83	78.23	78.90
4. Pachapolliam	11° 0'	77° 41'	909	76.3	1.63	74.90	2.06	.53	77.47	3.56	80.13	80.00
5. S.W. end Bangalore Base.	13° 1'	77° 37'	3116	72.5	3.07	73.50	0.94	.60	73.64	8.06	81.64	85.37
6. N.E. end Bangalore Base.	13° 4'	77° 42'	3007	72.9	3.04	73.61	0.43	.60	74.63	7.78	83.35	85.40
7. Nanthabadi	15° 6'	77° 36'	1319	87.7	3.16	73.15	2.47	.61	83.23	3.13	86.36	89.10
8. Kodungur	17° 8'	77° 41'	1072	81.8	2.15	80.82	4.72	.62	86.16	5.07	91.23	93.52
9. Damarguda	18° 3'	77° 43'	1934	73.4	3.79	85.20	+0.17	.83	86.19	4.37	91.16	96.64
10. Sontana	19° 5'	77° 42'	1711	71.9	3.31	90.64	-0.05	.68	91.37	4.43	95.70	98.12
11. Badgaon	20° 44'	77° 50'	1120	70.7	3.73	97.10	-0.50	.80	97.30	3.88	98.12	102.34
12. Elmadpur.	23° 36'	77° 43'	1681	84.9	3.85	96.65	+0.14	.83	96.08.65	4.38	102.00	107.34
13. Kahanpur	24° 7'	77° 42'	1765	80.3	2.13	86.01.11	4.04	.61	95.76	4.63	102.20	11.86
14. Pahargah.	24° 35'	77° 44'	1641	76.6	3.40	83.02	3.34	1.60	86.86	4.23	102.88	14.31
15. Utra	26° 37'	77° 40'	812	81.8	2.62	11.19	+4.68	.73	16.59	9.23	18.81	20.64
16. Dattair	28° 44'	77° 41'	719	65.7	3.42	24.13	-3.02	.73	31.81	1.83	33.66	36.31
17. Kallana (1866)	29° 31'	77° 42'	896	73.7	3.54	30.41	+0.61	1.04	32.36	3.13	34.38	36.51
18. Nohi	29° 53'	77° 43'	831	68.4	3.49	28.40	-6.47	.76	23.66	3.26	24.94	29.16
19. Dehra.	30° 30'	78° 6'	3259	61.9	3.00	23.85	-4.80	.77	18.63	3.99	24.71	30.45
20. Muscoria.	30° 38'	79° 13'	6020	69.6	23.86	00.66	-1.16	.69	00.43	10.23	35.67	31.93
Kallana (1870).			82.7	3.67	86.15.73	+5.11	+1.05	86.31.69	3.13	86.24.01
Coast Stations.												
21. Alleppy	9° 30'	76° 20'	6	80.0	1.72	85076.02	+3.61	+1.86	85081.23	0.02	85081.23	85079.90
22. Mangalore	12° 52'	74° 49'	7	79.7	1.43	80.13	3.76	.44	84.37	.83	84.39	86.01
23. Madras	13° 4'	80° 17'	27	74.4	1.82	63.71	1.16	.63	64.46	.07	64.47	65.37
24. Cochin	16° 56'	82° 16'	9	76.3	1.69	89.93	3.06	.85	93.66	.02	93.68	93.99
25. Calcutta.	22° 33'	88° 24'	16	79.9	3.16	86003.70	3.77	.62	86006.69	.05	86006.14	86007.29
Ocean Station.												
26. Minicoy	8° 17'	73° 3'	6	79.7	1.91	85078.07	+3.69	+0.35	85082.31	0.03	85082.33	85078.43
Base Station.												
27. Kew	51° 26'		54.2	1.37	-2.51	86113.98	.06	86114.03	86113.56

JAMES P. BASEVI, Captain R.E.,
Deputy Superintendent of Survey.

II. "On the Theory of Resonance." By the Hon. J. W. STRUTT.
Communicated by W. SPOTTISWOODE, F.R.S. Received July 2,
1870.

(Abstract.)

An attempt is here made to establish a general theory of a certain class of resonators, including most of those which occur in practice. When a mass of air or other gas is enclosed in a space bounded nearly all round by rigid walls, but communicating with the external air by one or more passages, there are certain natural periods of vibration or resonant notes whose determination is a matter of interest. If the dimension of the air-space is small compared to the wave-length of the vibration, the dynamics of the motion is, in its general character, of remarkable simplicity. It is for the most part under this limitation that the problem is considered in the present paper. The formula determining the resonant note is

$$n = \frac{a}{2\pi} \sqrt{\frac{c}{S}},$$

where n is the number of complete vibrations per second, a the velocity of sound, and S the capacity of the air-space; c is a quantity proved to be identical with the measure of electric conductivity between the interior of the vessel and the external space, on the supposition that the air is replaced by a uniform conducting mass of unit specific conducting-power, and the sides of the vessel and passages by insulators. When there is more than one passage the formula is still applicable according to the above definition of c ; and when the passages are sufficiently far apart not to interfere with each other, the resultant c is by the electrical law of parallel circuits simply the sum of the separate values for each passage considered by itself. When this condition is not satisfied the value of c , thus found by mere addition, is too great.

The question thus resolves itself into the determination of the conductivity (or the resistance which is its reciprocal) for different forms of passages or openings. The case of openings, which are mere holes in the sides of the vessel, has been already treated, although in a very different way, by Helmholtz, who, in his celebrated paper on vibrations in open pipes, compared his theory with the observations of Sondhauss and others on the notes produced, when such resonators are made to speak by a stream of air blown across the mouth. Sondhauss has also given an empirical formula applicable when the connecting passages are of the form of long cylindrical necks. These previous results are in agreement, as far as they go, with the formula here investigated, and which is applicable whatever may be the length of the neck. If L be the length and R the

radius, $\frac{1}{c}$ or the electrical resistance $= \frac{L + \frac{\pi}{2}R}{\pi R^2}$.

This supposes the neck a circular cylinder. If the section be an approximate circle of area σ , we may put

$$\frac{1}{c} = \frac{L}{\sigma} + \frac{1}{2} \sqrt{\frac{\pi}{\sigma}}.$$

When the neck is very long the second term may be neglected, and when L is very small the first term becomes insignificant. In the third part experiments are described which were instituted to compare the general formula with observation, and which gave a satisfactory agreement. The value given above for $\frac{1}{c}$ is only approximate. It is proved, however, that the resistance of a finite cylindrical conductor whose plane ends lie in two infinite insulating planes, but join on to conducting masses on the further side, corresponds to a length $L + a$ of the cylinder, where

$$a < 2.305 R \frac{10.615 - e^{-\frac{L}{R}}}{14.771 - e^{-\frac{L}{R}}} \\ > \frac{\pi}{2} R.$$

As a particular case, it appears that the correction to the length of an organ-pipe, supposed, as in Helmholtz's paper, to be surrounded at the mouth by a wide flange, lies between $.785 R$ and $.8282 R$.

Approximate formulæ are investigated for the resistance of tubes which are not exact circular cylinders. It will be sufficient to particularize here the case of tubes of revolution. The resistance is shown to lie between the two limits

$$\frac{1}{\pi} \int \frac{dx}{y^3}$$

and

$$\frac{1}{\pi} \int \frac{1}{y^3} \left\{ 1 + \frac{1}{2} \left(\frac{dy}{dx} \right)^2 \right\} dx,$$

where y denotes the radius of the tube at the point x .

When there is more than one vessel in the vibrating system, there are several independent periods of vibration corresponding to the degrees of freedom. The theory of these vibrations is also considered.

In the experimental part of the investigation the object is to determine with sufficient precision the pitch of the resonant note. This is generally done by causing the resonator to speak. For several reasons, which are detailed, I consider this course unsatisfactory, and have availed myself of other indications to fix the pitch, which are not, indeed, capable of so great an apparent precision, but yet are more to be depended on.

III. "On the Aromatic Cyanates." By A. W. HOFMANN,
LL.D., F.R.S. Received July 30, 1870.

The only member of this class which has hitherto been studied is the phenylic cyanate. About twenty years ago I discovered this compound among the products of a very complex reaction which took place on submitting to destructive distillation a substance which I then named oxamelanil or melanoximide*, but which at the present time would be considered as oxalyldiphenylguanidine. The phenylic cyanate, which I then called anilocyanic acid, is formed only in very small quantity; I never had more than a few grammes of the substance in my possession; and it was only from the sharply defined properties of the body that I was enabled to describe it correctly.

Eight years later I again met with this compound. When I found that diphenylsulphourea, by treatment with anhydrous phosphoric acid, split up into aniline and phenylic mustard-oil, the idea naturally suggested itself to utilize this reaction for the preparation of phenylic cyanate, by distilling normal diphenylurea with anhydrous phosphoric acid†.

In fact, phenylic cyanate can be prepared in this way. On heating dry diphenylcarbamide with anhydrous phosphoric acid, the frightful odour of the cyanate was immediately developed; and on distilling the mixture of the two bodies, the phenylic cyanate came over in colourless drops. When, however, the experiment was made on a somewhat larger scale, the product was so small that I was compelled to consider this process more as a mode of formation of the substance than as a method for its preparation.

Lately my experiments on the mustard-oils led me to a simple process for the preparation of the phenylic cyanate and its homologues.

In a former paper‡ I drew attention to the facility with which the mustard-oils combine with a molecule of alcohol. When phenylic mustard-oil is heated for a considerable time with alcohol, it yields the beautifully crystalline half sulphuretted phenylurethane, which, when distilled alone, or, still better, with phosphoric anhydride, again splits into its components alcohol and phenylic mustard-oil.

Taking into consideration the result of this experiment, ought we not to obtain the phenylic cyanate on distilling phenylurethane with phosphoric acid?

Phenyl Series.

Phenylurethane.—The phenylurethane is known; I had already obtained it in the above-mentioned research on the phenylic cyanate. When this substance was treated with methyl, ethyl, or amyl alcohol, the phenylurethane of the methyl, ethyl, or amyl series was obtained§. Since then

* Hofmann, Ann. Chem. Pharm. vol. lxxiv. p. 9.

† Hofmann, Proc. Roy. Soc. vol. ix. p. 275.

‡ Hofmann, Berichte Deutsch. Chem. Gesell. Jahrg. ii. S. 116.

§ Hofmann, Ann. Chem. Pharm. vol. lxxiv. p. 16.

the phenylurethane of the ethyl series, phenylcarbaminic ether, has been carefully investigated by Messrs. Wilm and Wischin*, who obtained it by the action of chlorocarbonic ether upon aniline.

I have repeated the experiment of Messrs. Wilm and Wischin, and can fully confirm their results. The body prepared in this manner is identical with that which I formerly obtained. The melting-point of the substance, after repeated crystallization, was found to be 51° ; Messrs. Wilm and Wischin gave $51^{\circ}5$ – 52° . The boiling-point was about 237° , the same as that found by those gentlemen.

Messrs. Wilm and Wischin state that the phenylcarbaminic ether (which they call carbanilidic ether) is volatile without decomposition. I find that although the greater portion escapes decomposition when distilled, yet part of it splits up into phenylic cyanate and alcohol,



which is perfectly in accordance with what I expected from the study of the half-sulphuretted phenylurethane. On distillation, the well-known and familiar odour of the phenylic cyanate is immediately developed; and, in fact, Messrs. Wilm and Wischin must also have observed it; for they say of the carbanilidic ether, "the vapour of this body excites a copious flow of tears, but when diffused has a faint resemblance to bitter almond-oil." What Messrs. Wilm and Wischin smelt was the phenylic cyanate. If the mixture of phenylic cyanate and alcohol, obtained along with a large quantity of phenylurethane by the distillation of the latter, be allowed to stand for twenty-four hours, the odour of the cyanate will have disappeared entirely; the cyanate and alcohol have recombined with formation of phenylurethane.

After these results of the behaviour of phenylurethane under the influence of heat, there could be no doubt that phenylic cyanate would be obtained by the employment of phosphoric anhydride.

Experiment has fully confirmed this expectation.

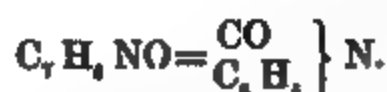
Phenylic Cyanate.—When phenylurethane is heated with anhydrous phosphoric acid, a considerable quantity of a colourless liquid distils over of great refractive power, having a pungent odour, and attacking the eyes strongly. This liquid is phenylic cyanate, which only requires redistillation to be obtained pure. As in all operations in the aromatic series in which phosphoric anhydride takes part, the quantity obtained is by no means that required by theory, although it approaches it.

For more than one reason I had desired to discover a method for the preparation of the phenylic cyanate. I was now enabled to determine more accurately the boiling-point of the body; it is 163° . In my former determination, for which only a few grammes were available, I had found it to be 178° .

The specific gravity of the phenylic cyanate at 15° is 1.092. Its vapour-

* Ann. Chem. Pharm. vol. cxlvii. p. 157.

density was determined in aniline vapour. The numbers obtained confirmed the formula already established by analysis.



	Theory.	Expt.
Specific gravity of vapour compared with hydrogen	59.50	58.90
„ „ „ compared with air.	4.13	4.09

With respect to the behaviour of the body with other substances, I can refer to my former paper. With water it yields carbonic acid and diphenylcarbamide; in contact with alcohol the urethane is reproduced; it combines immediately with ammonia and its derivatives, forming an inconceivable number of ureas. But, besides other combinations, it exhibits a remarkable reaction. I may mention that the possession of a considerable quantity of phenylic cyanate has given me an opportunity of again observing the behaviour of this body towards triethylphosphine, formerly described*. If a glass rod, moistened with the phosphorus base, be dipped into a considerable quantity of phenylic cyanate, in a few moments it becomes very hot, and the whole solidifies to a mass of beautiful crystals.

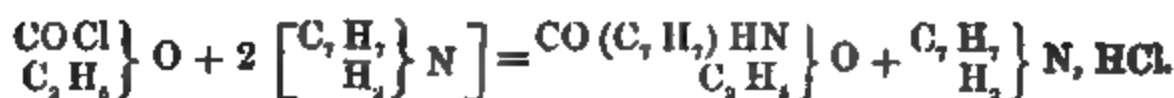
The principal product formed in this reaction is a body insoluble in water, and not very soluble in boiling alcohol, from which it crystallizes on cooling in fine prisms, which in an analysis formerly published gave numbers corresponding to cyanurate of phenyl, and may therefore be regarded as phenylic cyanurate. I must, however, leave it an open question, whether this substance is identical with the phenylic cyanurates, obtained the one by the action of chloride of cyanogen on phenol†, the other from triphenylmelamine‡.

Now that the necessary material can be obtained, there is no difficulty in a complete investigation of this body, as well as the by-products formed by the reaction in question.

We may here find room for some observations on the homologues of the phenylic cyanurate.

Tolyl Series.

Tolylurethane.—Chlorocarbonic ether acts with the greatest violence on toluidine, so that it is advisable to conduct the reaction in the presence of ether.



The ethereal solution, filtered from the toluidine hydrochlorate, on evaporation leaves the tolylurethane as an aromatic oil, which, when cooled by a

* Hofmann, Ann. Chem. Pharm. Suppl. vol. i. p. 57.

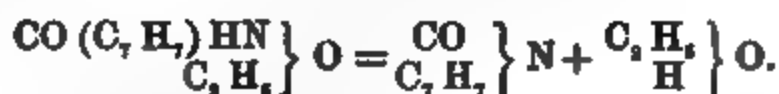
† Hofmann, Berichte Deutsch. Chem. Gesell. Jahrg. iii. 8. 273.

‡ Hofmann, Berichte Deutsch. Chem. Gesell. Jahrg. iii. 8. 274.

freezing-mixture, solidifies with difficulty, and, as a rule, only after standing some time.

Tolylurethane is insoluble in water, but dissolves with ease in alcohol and ether, and crystallizes from the former in fine long prisms, which melt at 32°.

Tolylic cyanate.—When distilled alone, the tolylurethane behaves like phenylurethane, the greater part passing over undecomposed, whilst a small portion is resolved into tolylic cyanate and alcohol.



When the distillation is performed in the presence of phosphoric anhydride, the alcohol is fixed, and the tolylic cyanate passes over in a nearly pure state. It only requires rectification to be perfectly pure. Tolylic cyanate is a colourless liquid, boiling at 185°, of high refractive power, and a powerful odour exciting a copious flow of tears.

A vapour-density determination in aniline vapour gave the following results:—

	Theory.	Expt.	
		I.	II.
Specific gravity of vapour compared with hydrogen	66.5	64.6	65.7
Specific gravity of vapour compared with air	4.61	4.48	4.56

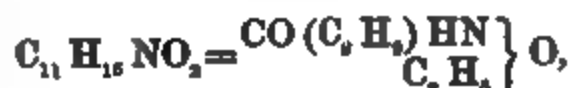
Tolylic cyanate behaves towards water and ammonia and their derivatives like phenylic cyanate. On treatment with water ditolylurea is produced with evolution of carbonic acid; with the alcohols it forms the corresponding nrethanes, and with ammonia and the amines it yields a group of compound ureas.

Triethylphosphine produces the same change as in the phenyl-compound; it takes place, however, somewhat more slowly. The very beautiful crystalline compound thus formed I hope soon to be able to investigate.

Xylyl Series.

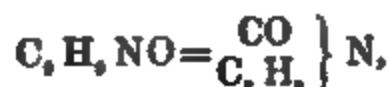
The experiments were precisely similar to those in the phenyl and tolyl series. The reaction with xyloidine is, however, somewhat more sluggish than with aniline and toluidine.

Xylylurethane,



crystallizes in fine needles, which melt at 58°.

Xylylic cyanate,



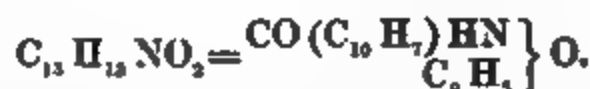
is a colourless, highly refractive liquid of feeble odour, and attacking the eyes but slightly. The boiling-point is about 200°. Its vapour-density was taken in aniline vapour.

	Theory.	Expt.
Specific gravity of vapour compared with hydrogen	73.50	74.69
" " " compared with air	5.10	5.18

Xylylic cyanate exhibits the same reactions as those which are so prominent in the corresponding members of the phenyl and tolyl series, but somewhat weaker. The combinations which take place almost immediately in the case of phenylic and tolylic cyanates often require days with the xylylic cyanate. Even in the presence of triethylphosphine xylylic cyanate solidifies but slowly.

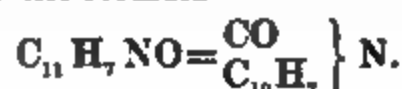
Naphthyl Series.

Naphthylurethane.—The formation of this analogue of urethane was effected by the action of chlorocarbonic ether on naphthylamine. It is insoluble in water, more soluble in alcohol, and crystallizes from the latter in needles which melt at 79°. Its composition is expressed by the formula



Naphthylic cyanate.—A short notice of this compound has already appeared. When I had found that diphenylcarbamide gave phenylic cyanate by distillation with phosphoric anhydride, Mr. Vincent Hall* made the corresponding experiment in the naphthyl series, but only obtained a very small quantity of the naphthyl compound. The formation of the naphthylic cyanate by this method was, however, established.

The naphthylic cyanate is obtained in considerable quantity by the distillation of naphthylurethane with phosphoric anhydride. It is a colourless, not very mobile fluid, whose boiling-point is about 269°–270°. Its vapour has the pungent odour which is peculiar to the cyanates, but at the ordinary temperature naphthylic cyanate is almost odourless. Its composition is expressed by the formula



I have, moreover, satisfied myself of its correctness by the reactions of the body. On considering its behaviour with water and alcohol, and their derivatives, there can be no doubt as to the nature of the compound. The facility with which these reactions take place with the naphthyl compound is remarkable. The naphthylic cyanate acts with incomparably greater quickness and precision than the analogous xylyl-compound; this is particularly shown in the action of triethylphosphine, which causes the cyanate of the naphthyl series to solidify almost instantaneously.

I have to give my best thanks to Mr. F. Hobrecker for his efficient assistance in the prosecution of the above research.

* Proc. Roy. Soc. vol. ix. p. 365.

November 30, 1870.

ANNIVERSARY MEETING.

General Sir EDWARD SABINE, K.C.B., President, in the Chair.

General Boileau, for the Auditors of the Treasurer's Accounts on the part of the Society, reported that the total receipts during the past year, including a balance of £324 4s. 7d. carried from the preceding year, and £800 taken from the Oliveira bequest on deposit, amount to £5410 3s., and that the total expenditure in the same period amounts to £5282 13s. 9d., leaving a balance of £91 18s. 3d. at the Bankers, and of £35 11s. in the hands of the Treasurer.

The thanks of the Society were voted to Mr. Spottiswoode and the Auditors.

The Secretary read the following Lists:—

Fellows deceased since the last Anniversary.

Royal.

His Imperial and Royal Highness Leopold the Second.

On the Home List.

Edward William Brayley, Esq.
Alexander Bryson, M.D.
The Hon. and Rev. Richard Carleton, M.A.
Sir James Clark, Bart., M.D.
Charles Collier, M.D.
James Copland, M.D.
John Thomas Graves, Esq., M.A.
William Alexander Mackinnon, Esq., M.A.
James Prince Lee, Lord Bishop of Manchester.

Augustus Matthiessen, Esq., Ph.D.
William Allen Miller, M.D., D.C.L.,
Treas. R.S.
Arthur Morgan, Esq.
The Right Hon. Sir Frederick Pollock, Bart., M.A.
The Rev. William Taylor.
James Vetch, Capt. R.E.
Augustus V. Waller, M.D.
John Wilson, M.D.

On the Foreign List.

Heinrich Gustav Magnus.

Change of Title.

The Bishop of Oxford to Bishop of Winchester.

Fellows elected since the last Anniversary.

William Froude, C.E.	Capt. Robert Mann Parsons, R.E.
Edward Headlam Greenhow, M.D.	William Henry Ransom, M.D.
James Jago, M.D.	George Granville William Sutherland-Leveson-Gower, Duke of Sutherland, K.G.
Neril Story Maskelyne, M.D.	Robert H. Scott, Esq.
Maxwell Tylden Masters, M.D.	George Frederic Verdon, C.B.
Robert, Lord Napier of Magdala.	Augustus Voelcker, Ph.D.
Alfred Newton, M.A.	Samuel Wilks, M.D.
Andrew Noble, Esq.	
Capt. Sherard Osborn, R.N.	
Rev. Stephen Parkinson, B.D.	

On the Foreign List.

Anders Jöns Ångström. Joseph Antoine Ferdinand Plateau.

The President then addressed the Society as follows :—

GENTLEMEN,

SINCE our last Anniversary, another volume, the fourth, of the Society's Catalogue of Scientific Papers, has been published, bringing the alphabetical list of titles down to P O Z. Pursuant to the arrangement which I announced last year, Professor Carus of Leipsic spent some weeks in London during the spring, in preparing the Index Rerum. It was his intention to come again in the autumn to continue his labours; but the outbreak of the war prevented him from leaving his home and family, so that the resumption of his work is postponed for the present. The preparation of the Index Rerum will, however, continue to engage the careful attention of the Library Committee; meanwhile I am glad to say that good progress is being made with the printing of Volume V., of which a fourth part is now in type.

In the last summer we have had the misfortune of losing our Treasurer, Dr. William Allen Miller, one of our most valued and distinguished Fellows, and one who surely enjoyed the regard, as well as the high esteem, of all who knew him. He was also one who, in addition to important direct scientific work, had for several years shared actively in the cares and the labours called for by the many and varying subjects referred each year to the Royal Society, as represented by its Officers and Council, and by the special Committees appointed by them to deal with each case as it arises.

It may be in the recollection of some of the Fellows that, in the Anniversary Address delivered in 1863, I ventured to suggest the interest and probable value of a series of Pendulum Experiments at the principal stations of the Great Indian Arc. Encouraged by the warm concurrence in opinion and promised support of Colonel Walker, Superintendent of the Indian Trigonometrical Survey, a circular note was addressed by myself, with the concurrence of the Council, to several of the Fellows of the Royal Society conversant with the subject; and the correspondence, including an outline of the proceedings which in Colonel Walker's judgment would be required in India, should the experiments receive the sanction of the Indian authorities, was submitted through the proper channels to the Secretary of State for India, and received the sanction of the Indian Government.

The pendulums and accompanying apparatus having been prepared at the observatory of the British Association at Kew (aided by a special subsidy from the Government-Grant Fund placed annually at the disposal of the Royal Society), were embarked for India, in March 1865, under the charge of Captain Basevi of the Royal Engineers, appointed to conduct the experiments with them in India. These have been executed in great part, and are still in progress, under that officer's superintendence. In the meantime the Royal Society has been favoured by a recent communication from the Secretary of State for India, enclosing Col. Walker's official report of what has been accomplished, and of what remains to be accomplished in India, to complete the original programme—and transmitting a despatch signed by the Governor General and other high authorities of the Indian Government, requesting that the President and Council of the Royal Society may be invited to suggest at an early day any supplementary measures which they may deem desirable.

It appears that, at the date of Colonel Walker's report, pendulum experiments had been completed at twenty-five stations on the mainland of India, extending from Cape Comorin in lat. $8^{\circ} 5' N.$ to Mussoorie in lat. $30^{\circ} 28' N.$, and at a 26th station, on the Island of Minicoy, midway between the Maldives and the Laccadives, in lat. $8^{\circ} 6'$ —and that five additional stations had been allotted as the work of the present year, four of which are in the high tablelands of India between 32° and $36^{\circ} N.$, and one near the mouth of the Indus.

I had the pleasure of receiving, about the same time, a letter from Colonel Walker himself, announcing his intention of being in England, on furlough, towards the end of December, when he might be personally informed of any further uses to be made of the pendulums in India—proposing also that, on the return voyage to England, Captain Basevi should be instructed to obtain the rates of the pendulums at Aden (suggested as desirable, from its geographical peculiarities, by our Foreign Secretary, in a letter to myself, printed in the Minutes of Council of June 1864), and at a station to be selected in the vicinity of the Bitter Lakes. Colonel Walker also suggested that, on the arrival of the pendulums in England,

their rates should be ascertained at the Royal Observatory at Greenwich, in addition to the necessary repetition of the original experiments at the Kew Observatory.

The documents thus referred to having been read at a recent evening meeting, and suitable communications having been made to those Fellows of the Society who participated in the original recommendation of the experiments, a Committee has been named, with Colonel Walker as one of its members, to meet as soon as may be convenient after his arrival in England, to prepare a reply to the Indian Government.

It may perhaps be permissible to notice, on this occasion, that the experiments on the retardation which pendulums experience when vibrated in different gases, for which a sum was allotted some years since by the Government-Grant Committee, yet await the supervision of an experimentalist having sufficient leisure and interest in the subject. The apparatus at Kew may be made quite suitable for the purpose; and the occasion is favourable.

The successful voyage and safe return of the North-German Polar Expedition is an event on which the Royal Society may add its sincere congratulations to those of the public at large. The progress of geographical discovery, connecting with itself, as it does, the advancement of many physical sciences which require, either for extension or for confirmation, the assurance derived from experimental research, has for nearly three centuries been carried on in the Arctic Regions, where it has afforded a common field for the enterprises of the British and of the northern continental nations—enterprises conducive to hardihood, and to qualities which are the result of a generous emulation, unmingled with the deteriorating influences which are but too apt to be generated by the rivalries of war. The earliest of these undertakings were indeed antecedent to the existence of the Royal Society; but on their revival—which took place in 1818, at the termination of the great War by which Europe had been desolated for so many years—the British Government, at the instigation of the Royal Society, was the first to recommence, and for several years to continue, a succession of undertakings, in which we have now to recognize the successful participation of more than one kindred and allied people.

The German expedition consisted of the ‘Germania,’ a steamer of about 80 tons, duly strengthened for encounters with ice, commanded by Captain Koldewey, who seems to have possessed in an eminent degree the special qualifications required in such an enterprise. Besides her naval complement of twelve officers and seamen, four gentlemen were embarked for special scientific services, to whom were confided, among other objects, the researches preliminary to the measurement of an arc of the meridian on the coast-line of East Greenland. The ‘Germania’ was accompanied by a smaller vessel, not furnished with steam apparatus, named, “The

Hansa." Thus unequally provided, the two vessels parted company in the ice which has to be traversed in the passage from Europe to East Greenland, and did not subsequently rejoin; the 'Hansa' having been unable to force her way through the ice, was finally wrecked by it, the crew escaping on the ice, and being conveyed, on a constantly lessening ice-raft, to near the latitude of Cape Farewell, whence they made their way in their boats, which they had preserved, to the nearest Danish settlements, from which they have returned without loss of life.

The 'Germania' having forced her way through the ice by the aid of steam, anchored on the 5th of August, 1869, in the small but secure bay, in lat. $74^{\circ} 32'$, and long. $18^{\circ} 53' W.$, on the south side of the island on which my pendulum experiments had been made forty-six years before. They subsequently visited Cape Philip Broke in lat. $74^{\circ} 55'$, which had been the first landing-point on the coast when examined by Captain Clavering and myself in the British Expedition of 1823. The highest latitude reached by the 'Germania' was $75^{\circ} 31'$, where she was stopped by the ice, and returned to winter in the bay, in the Pendulum Island, in which they had first anchored. The extreme northern point of the coast which had been seen in 1823, named by Captain Clavering "The Haystack," in lat. $75^{\circ} 42'$, was visited in sledges in the spring of 1870, by parties engaged in the survey operations, which extended into the 77th parallel.

One of the most noteworthy results of this expedition, and it may possibly prove one of the most important, is the discovery that the lands adjacent to the bays and fiords in East Greenland abound in flocks of Reindeer and Musk-oxen, and in smaller game of various descriptions. It is stated that during the stay of many months on the coast the crew were rarely without an abundant supply of fresh food, derived from the country itself. It is possible that this abundance of game, combined with the magnificence of the glaciers and of the mountain scenery accessible by the deeply intersecting fiords, may tempt persons who in these days have trained themselves to arduous mountain-ascents to visit a country in which panoramic views over yet unexplored regions of the globe might be a recompense for their toils and dangers. The mountains rising from one of the fiords visited by the surveying parties of the 'Germania' attain a height of 14,000 feet. The view from such an elevation might possibly accomplish more than many maritime expeditions, to shape out the yet unknown geography of Northern Greenland*. The distance in a direct line from the most northern point of the coast visited by the surveying parties of the 'Germania,' to the expanse of open ocean seen by Kane and Hayes to the north of Kennedy's Channel, and described as abounding in animal life and

* The officers of the 'Germania' speak with enthusiasm of the scenery in the deep fiord in lat. 73° , up which they steamed more than 70 nautical miles in a westerly direction. They say :—"The further we went the milder we found the temperature; the scenery was grand as in the Alps. The true interior of Greenland showed itself with constantly increasing grandeur and beauty to our admiring eyes."

indicating in many ways the proximity of an extensive open sea, is not more than about 400 geographical miles.

The Survey which has been recently made under the auspices of the Swedish Government, with a view to the measurement of an arc of the meridian at Spitzbergen, may have suggested the corresponding survey of the coast of East Greenland. But, without doubt, the extent of continuous land in the direction of the meridian is much greater in Greenland than in Spitzbergen; whilst the fiords, which so generally characterize the Greenland coast, would probably greatly facilitate the access to localities suitable for trigonometrical stations. The return of the '*Germania*,' whilst the objects of her mission were still incomplete, was occasioned by the necessity of replacing her boiler, which had wholly failed; and she will probably resume her operations in the coming year. It is understood that the Swedes also are organizing another Spitzbergen expedition, of which the special objects have not yet been announced. It may be hoped that the remeasurement of the Fairhaven Hill may be remembered on this occasion. Its height in 1823 was determined with great care, both geometrically and barometrically, by Captain Foster and myself (*Phil. Trans.* 1824, Art. xvi.). The permanency, or otherwise, in the elevation of marked features of the land is a subject of considerable geological importance in some countries; and Spitzbergen is one of those in which it has been occasionally questioned.

The discrepancy in the action of the currents experienced in 1823 and 1869, on the extensive ice-fields which occupy the middle space between the coasts of Europe and of East Greenland, is very noteworthy. In 1823 the most careful observations, of officers greatly practised in such investigations, failed to discover any perceptible surface-current whatsoever, either by its effect on the ice itself, or on the surface-water of the sea between the ice and the Greenland coast; whilst in 1869 the crew of the '*Hansa*,' having taken refuge on the ice after the loss of their vessel, in the approximate geographical position of $70^{\circ} 50' \text{ N.}$ and 21° W. , were carried by it to the lat. of $61^{\circ} 12' \text{ N.}$ and long. (about) 42° W. ,—being a drift extending over more than 500 geographical miles, accomplished in little less than 200 days, the average being somewhat less than three miles a day. In seas much encumbered by floating ice, currents are generally ascribed to the prevailing winds; and there appears to have been in 1869 a considerable amount of northerly gales. But the frequent existence of a current setting to the south and south-west down the coast of East Greenland has been recognized by the highest authorities, and is regarded by Forchhammer, in his valuable memoir on the Phenomena of the Sea, in the *Philosophical Transactions* for 1865, as a returning branch of the Gulf-stream, recognizable as such by the difference in the analysis of the polar and equatorial waters. This branch of physical research has doubtless received full attention from the officers of the '*Germania*;' and we must await the publication of the complete account of the voyage for the full details.

The plant-remains of the Miocene epoch, discovered in Spitzbergen and

Greenland, have given a new feature of interest to future explorations within the Arctic Circle. The valuable memoir which Professor Oswald Heer, of Zurich, has contributed recently to the Philosophical Transactions (1869, Art. XIII.), has added much to the evidence contained in his previous papers of the existence, at that early period of the earth's history, of a vegetation in some cases identical with, and in others scarcely differing from, that which now lives and flourishes in the Temperate Zone—a vegetation comprehending oaks, planes, chestnuts, and even a Magnolia, the leaves and fruit of which were found in the North-Greenland deposits. Altogether, Professor Heer has identified no less than 137 species of the Arctic flora of the Miocene age; and he has moreover inferred, with great appearance of reason, that at the same era vegetation of the same character may have prevailed generally in lands within the Arctic Circle. The anticipation of future discoveries of plant-remains, adding possibly largely to the number of 137 species already recognized, must tend to give to land-explorations and excursions an interest which was comparatively wanting to them when all that the explorer could anywhere hope to find (other than the scanty, though in some respects beautiful, flora which the rigours of the Arctic region at the present time still suffer to exist) was, at most, the less attractive fossil remains of much earlier geological ages, to the climatology of which less interest attaches than to that of the comparatively recent (however ancient) Miocene age.

There have been in the past year two vacancies in the list of Foreign Members of the Royal Society. The two gentlemen who have been elected are Professor Joseph Antoine Ferdinand Plateau, of Ghent, and Professor Anders Jöns Ångström, of Upsala. Professor Plateau has been an earnest worker in the field of physical science for above forty years. His memoirs, the titles of which (forty-six in number) are given in the fourth volume of the Society's Catalogue of Scientific Papers, give evidence of the completeness with which he has treated the subjects he has taken up; and many of his experiments are remarkable for their ingenuity and originality. Questions in physical optics were the first to engage his attention. In this branch of science his researches were confined to the laws of visual appearances, including those relating to ocular spectra, the duration of impressions on the retina, and irradiation; and though some of his conclusions may have to be corrected, there is no doubt that in this special department he has done more than any one of his predecessors or contemporaries. Another subject which has occupied him in his later years, and has supplied the materials of eleven memoirs, concerns the figures of equilibrium of a liquid mass without gravity. No one can contest the perfect originality of this series of investigations, or fail to admire the simple and effective means by which he has carried out his experiments, and the sagacity with which he has arrived at results which have formed a new starting-

point for mathematical investigations relating to the corpuscular theory. It is much to be regretted that Professor Plateau's scientific labours for more than twenty years past have been carried on under a deprivation of sight occasioned by the ardent pursuit of his favourite science. Although totally blind, his experiments are of the most exquisite delicacy; and the reasoning of which they form the material is as accurate and penetrating as the experiments themselves are beautiful.

The second gentleman who is elected a Foreign Member, Professor Ångström, is distinguished by his researches in many departments of physical science, and is entitled to rank among the very highest of those who have within a few years developed the powers of spectroscopy to their present marvellous extent. In fact, his 'Optic Researches,' 1853, contain the fundamental principles of nearly all that has been done since.

The following notice is from the pen of the Rev. Dr. T. Romney Robinson, F.R.S., of Armagh :—

"From Euler's theory of Resonance, he infers that, as a body absorbs all the series of oscillations which it can itself assume, it must, when heated so as to become luminous, emit the same rays which at a lower temperature it absorbs.

"It is hardly possible to announce more distinctly this important fact, on which nearly all solar and stellar spectroscopy rests; and it is much to be regretted that, from the ignorance of the Scandinavian languages which is so general in this country, it was not known here till 1856. He proceeds to examine the spectrum of the electric spark. It is traversed by two sets of lines on an obscure ground. The first of these belongs to air. The lines reach across the spectrum, and do not vary with the nature of the electrodes. The second set come from the electrodes; they are in general far brighter than the other, and, unless the discharge is powerful, are confined to the vicinity of the electrodes. Each metal has its own set; but, owing to the small dispersion of his spectroscope (one prism of 46°), he thought some lines were common to two or more metals. Alloys showed the lines of their components with no greater difference than could be explained by the want of power in his instrument. He shows that these lines are not produced by interference. The solar spectrum may be regarded as a reversion of the electric one; and he is convinced that the explanation of the dark lines in the former embraces that of the bright lines in the other. Each gas has its peculiar spectrum; he notices the faintness of the oxygen-lines in air, and he describes the three bright bands of hydrogen, and a fourth fainter.

"In 1861, in a second memoir, he developed his theory of the reversion of the lines. He had then obtained a better spectroscope and a large Ruhmkorff, with which he compared many of the solar with metallic lines. Many double lines belong to two metals. Gas-lines are more diffused and less sharply defined than those of metals, especially metals difficult of fusion. He found no evidence of nitrogen or oxygen in the sun.

"His third memoir (1863) contains a determination of the wave-lengths of Fraunhofer's principal lines, and others, 70 in all. As the same matter is treated far more completely in a subsequent memoir, it is only necessary to notice that he corrects his measures for the pressure and temperature of the air, for the temperature of the grating employed, and for the aberration caused by a motion of the instrument in a direction at right angles to the incident light. This last will cause a difference between the deviations on each side of zero. In the high latitude of Upsala, and in an unfavourable summer, the observations which he made to test the reality of this correction were neither sufficiently good nor numerous to satisfy him; yet the difference which he found agreed very closely with what was computed.

"A memoir (1865) gives the places and map of the solar lines beyond G, where Kirchhoff's map terminates. His spectroscope-arrangements are original. He uses two 'astronomic telescopes' (probably of $3\frac{1}{2}$ -inch aperture) as collimator and observing-telescope, and a single bisulphide-of-carbon prism of 60° . The large aperture of the telescopes gives illumination enough to permit the use of a high magnifying-power; and this compensates for a less dispersion*; so that the apparatus showed all Kirchhoff's lines with great distinctness. For extreme violet rays an image of the sun was formed on the slit by an object-glass like that of the telescope. The metallic spectra were obtained, not by an induction-machine, but by the voltaic discharge of a battery of 50 Bunsens between massive electrodes of each metal. It may be that in this way a higher temperature is attained than by the induction-spark (especially if Wilde's instrument were substituted for the battery); for he found that more lines, and of higher intensity, were thus brought out than even by the large Ruhmkorff.

"He thus found in the solar spectrum 390 iron-lines more than were previously known. He also found lines of manganese, and ascertained the identity of the line λ with the fourth hydrogen-line which he had previously discovered.

"In 1866, he found that the lines A α B, and some others, though telluric, are not due to aqueous vapour; for they are of undiminished strength at 16° below zero of Fahrenheit, while the other telluric lines were scarcely visible.

"In 1869 his researches on the solar spectrum contained determinations of wave-lengths. These were made by means of four gratings by Nobert, celebrated for his microscopic test-lines. These determinations appear to be exceedingly accurate. The intervals of the gratings were from $\frac{1}{3281}$ inch to $\frac{1}{3636}$, and were determined with extreme care.

"They were compared by a microscope of 200 power, carried by a dividing-engine carefully verified with a metre which had been compared with the standard platinum one at the Conservatoire des Arts et M \acute{e} tiers.

"The corrections already mentioned for pressure, temperature, and aber-

* "Wers found that, with a telescope of 4 inches aperture, and flint prism of the same dimensions, 60° , the line between the two of D was visible."

"ration were attended to ; and in the measures of the deviations the 5th
"and 6th spectra were generally taken ; his experience indicates that in
"taking these measures it is best to have the intervals of the grating not
"very close, but covering a surface of some magnitude : the deviations are
"less, but the spectra brighter.

"The result of these measures is given in six beautiful maps, extending
"from B to H². The places of the lines are laid down on a scale, not of de-
"viations, but of wave-lengths, in which the unit is a ten-millionth of a
"millimetre.

"The advantage of this can scarcely be overrated ; for in other maps
"the scale depends on the refractive indices of the prisms and their adjust-
"ments, so that no two are comparable ; while at ordinary temperatures and
"pressures the wave-lengths are invariable.

"In addition to hydrogen he found in the sun thirteen metals, of which
"titanium has 200 lines. The total amount of metallic lines is nearly 800,
"including most of the strong lines ; so that probably the sun has few ele-
"ments which are not found on our earth. There are some strong lines
"between E and G whose origin as yet is unknown ; one coincides with a
"line of bromine, whose presence is not probable. There are three strong
"magnesium lines not present in the sun ; and oxygen, nitrogen, and carbon,
"so common here, are not detected there. He thinks the sun is not hot
"enough to make the first two luminous ; carbon in the circuit of his battery
"shows no lines of its own, but those of its compounds. He doubts
"Plücker's notion of the same substance having different spectra, and
"thinks that the change is merely due to the increased temperature, which
"makes faint lines intense—and is disposed to think that the pillar-like
"appearance observed in some star-spectra indicates a lower temperature
"than the black sharp lines which occur in others.

"The aurora and zodiacal light have in common a line ($\lambda=5567$) whose
"origin is yet unknown.

"It must be admitted that this list is the exponent of intellectual power
"of first-rate excellence, and of additions to our knowledge which are not
"only intrinsically valuable, but are also eminently suggestive of ulterior
"progress. Nor can his claim to priority of entrance into this wondrous
"region be disputed by any unprejudiced judge. Though Stokes and Thom-
"son nearly at the same time drew the same conclusion as to absorption,
"and even satisfied themselves of the identity of D with the sodium lines—
"though Stewart, and still more successfully Kirchhoff, again brought it
"before the public three years after, yet this is what happens to every
"inventor or discoverer : others follow in his track, some perhaps more
"successfully than himself ; but his right remains untouched."

The 'Porcupine' having been again placed by the Admiralty (for three months of the summer of 1870) at the disposal of the physicists and naturalists engaged in researches on the temperature of the sea at great depths, and on the nature of the sea-bottom and of the life existing in its

vicinity, the coasts of Spain and Portugal and of the Mediterranean have been the fields of exploration in the present year. As the results will form the subject of a communication to the Society at its next Evening Meeting, I abstain from advertng on the present occasion to any of the interesting particulars with which Dr. Carpenter and Mr. Gwyn Jeffreys have been so obliging as to furnish me.

Her Majesty's Government having been pleased to accede to a request made by a Joint Committee of the Royal and of the Royal Astronomical Societies, for means of conveyance to and from Cadiz and Gibraltar of twenty-five persons desirous of observing the Total Solar Eclipse on the 21st December 1870, and for a sum not exceeding £3000 to defray cost of instruments, travelling-expenses of the scientific party, and other miscellaneous services and purposes connected therewith, the necessary arrangements have been confided to an organizing Committee, under the immediate direction of the Astronomer Royal, and are now in progress.

You are perhaps aware that your President occupies, as such, an official seat in the Board of Trustees of the British Museum. In attending to these duties, I became impressed with the benefits which might result if objects, for the exhibition of which space cannot be found at the British Museum—and most especially specimens of natural history—were freely lent, under suitable regulations for their safe custody and safe return when required, to Provincial Museums. For this purpose it would be necessary to introduce a modification in the Act of Parliament, under which the Trustees are now bound not to permit the removal from the Museum of any article which has been once received into it. I have made this suggestion known in different quarters, and have found it, generally speaking, so favourably received, that I even thought it possible that I might be able on this occasion to inform you that some advance had been made in its actual realization. I can only say at present that some steps have been taken in that direction, which, I hope, may yet bear fruit.

I proceed to the award of the Medals.

The Copley Medal has been awarded to Mr. James Prescott Joule, for his Experimental Researches on Heat.

The researches, for which the Copley Medal has thus been awarded, are the same for which a Royal Medal was awarded to Mr. Joule in the year 1850. The terms of the award in 1850 were for "his Researches on the Mechanical Equivalent of Heat;" those on the present occasion were for his "Experimental Researches on Heat." Both awards refer to the same experiments, and are substantially for the same great step in Natural Philosophy; of which, therefore, it is needless for me to give you an account at this time. You are all aware that by it a great principle has been added to the sum of human knowledge—one fruitful in consequences in a thousand ways, and which, being accepted amongst undisputed truths, is now em-

bodied, without question, alike in the most wide-ranging speculations and the most matter-of-fact practice.

The award of two Medals for the same researches is an exceedingly rare proceeding in our Society, and rightly so. The Council have on this occasion desired to mark by it, in the most emphatic manner, their sense of the special and original character, and high desert of Mr. Joule's discovery. No words of mine could add to the value of the award.

MR. JOULE,

I present you with this Medal, in testimony of the very high sense which the Royal Society entertains of your researches, and of the results to which they have led.

The Council has awarded a Royal Medal to Professor William Hallowes Miller, Foreign Secretary of the Royal Society, for his researches and writings on Mineralogy and Crystallography, and for his scientific labours in the restoration of the National Standard of Weight.

These last-named labours appeared to me to have, from their national importance conjoined with great scientific merit, so peculiar a claim to a Royal Medal, that it was preeminently on this ground that I ventured myself to propose the award; and for the same reason I have obtained permission from the Astronomer Royal, who is the Chairman of the Standards Commission, to give here in full a statement from his pen, in which, as a Member of that Commission, I most entirely concur.

"For full explanation of the considerations which have guided the Council
"in awarding a Royal Medal to Professor Miller, it appears necessary to
"advert to some circumstances occurring at a time anterior to that which is
"recognized as limiting the claims that can be examined in reference to the
"award of a Medal. When, after the fire which destroyed the Houses of
"Parliament, it was ascertained that the National Standards of Length and
"Weight (then preserved in the Parliament-buildings) were entirely ruined,
"a Commission was appointed to consider the questions connected with the
"Restoration of the Standards. Although Professor Miller was not a
"Member of that Commission, his friendly assistance contributed greatly to
"guide the Commission in some of their more important recommendations,
"especially in those which related to the means to be provided for con-
"tingent restoration of the Standard of Weight. The Commission proposed
"in their Report (among a number of less conspicuous regulations), the
"repeal of the law providing a physical principle of restoration of the
"standards, substituting for it a reference to existing copies, and the esta-
"blishment of a Department of Standards. A Commission was then ap-
"pointed for construction of the New National Standards; and the services
"of Professor Miller, as Member of this Commission, were invaluable. His
"paper in the Philosophical Transactions, describing the operations for re-
"storing the value of the Old Standard of Weight, for constructing the New
"Standard of a different value, for constructing various derived Standards,

"and for establishing the relative value of the kilogramme, will long be
 "cited as a model of accuracy. To this, however, I refer at the present
 "time as introductory to the strict subject of this Medal.

"At length the principal object for which the successive Commissions
 "(and none of the Members more strongly than Professor Miller) had for
 "so many years so earnestly contended, namely the establishment of a De-
 "partment of Standards as a substantive branch of the Government (subor-
 "dinate to the Board of Trade), was attained. And, nearly at the same
 "time, a new Royal Commission, of which Professor Miller was a member,
 "was appointed for examining and reporting on the state of the Secondary
 "Standards, and for considering every question which could affect the Pri-
 "mary, Secondary, and Local Standards. As Head of the new Department,
 "the Government were most fortunate in being able to avail themselves of
 "the talent and energy of Mr. H. W. Chisholm; and to the cordial co-
 "operation of Professor Miller and Mr. Chisholm, principally in regard to
 "the questions of Weights, but also on many points applying to lengths
 "and capacities, is to be ascribed in great measure a result which (in the
 "hope of Professor Miller's colleagues) will prove to be an important legis-
 "lative success. Without the extensive knowledge and the long experience
 "of Professor Miller this result could not have been expected. The field of
 "labour was very wide; not only were the state of Standards of all degrees
 "of subordination, and the legislation respecting them, to be considered, but
 "the Commission and Professor Miller, in a high degree, had the serious
 "and responsible charge of offering to the Government their matured opi-
 "nions on the grave questions of introduction of Metric System, abrogation
 "of Troy Weight, and future arrangement of the entire system of British
 "Standards. It is believed that the fruits of these labours may be correctly
 "stated as:—the establishment of an office which for accuracy of standards
 "and perfection in the methods of using them, may compare favourably
 "with any in the world; the indication of the best direction of legislation
 "in establishment of regulations for their national utility; and the expo-
 "sition of the broad views which may advantageously be adopted by nations,
 "especially by Britain, in deciding on the course to be followed under the
 "competing claims of different systems. For this presumed success the
 "country is greatly indebted to the ability, the science, and the incessant
 "attention of Professor Miller."

Though in awarding the Royal Medal to Professor Miller it was natu-
 rally impossible not to advert to them, I am not prepared to give you any
 corresponding account of those researches in Mineralogy and Crystallo-
 graphy which, having been prosecuted for more than thirty years, have
 gained for him the highest reputation wherever those departments of
 knowledge are cultivated throughout the world, and have accordingly been
 translated into different European languages.

PROFESSOR MILLER,

It is with very great pleasure that I present you with this Medal, in
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recognition of the valuable services which you have rendered to your country, as well as to science.

The Council has awarded a Royal medal to Mr. Thomas Davidson, for his works on the 'Recent and Fossil Brachiopoda,' more especially for his series of monographs in the publications of the Palaeontographical Society.

The publications referred to in this award extend over a period of twenty-three years, viz. from 1847 to 1870; the plates which illustrate these works have been drawn by himself upon stone, and freely presented to the different Societies which have published his writings.

MR. DAVIDSON,

I have the pleasure of presenting this Medal to you in testimony of the value which the Royal Society attaches to your writings on British and Foreign Brachiopoda.

The Council has awarded the Rumford Medal to Monsieur Alfred Olivier Des Cloizeaux, for his researches in Mineralogical Optics.

For nearly thirty years M. Des Cloizeaux has been occupied in investigating the characters of crystallized bodies, and more especially those which are produced by their action upon light. He has subjected nearly 500 crystalline species to an optical scrutiny, determining the ratio of the velocity of light of various colours in air to its velocity in the direction of each of the axes of optical elasticity within the crystal, the angle between the optic axes in biaxial crystals, and the dispersion of the optic axes. The results of these observations are published in vols. xi. and xiv. of the '*Annales des Mines*,' and in vol. xviii. of the '*Mémoires des Savants Étrangers*'.

Assuming the truth of the laws connecting the optical properties of a crystal and its system of crystallization established by Sir David Brewster, he has applied them to correct the descriptions of mineral species, by the methods detailed in his '*Mémoire sur l'emploi du microscope polarisant et sur l'étude des propriétés optiques biréfringentes propres à déterminer le système cristallin dans les cristaux naturels ou artificiels*.' Numerous instances of these corrected descriptions occur in the '*Comptes Rendus*,' the '*Annales de Chimie*,' the '*Annales des Mines*,' the '*Transactions of the Royal Society*' for 1868, and in his '*Treatise on Mineralogy*.'

While investigating the effect of heat in modifying the action of crystalline bodies, he observed that the positions of the optic axes of felspar, chrysoberyl, and brookite changed on the application of heat, returning to their original positions when the crystal regained its initial temperature, as indeed was known before in the case of felspar; but on increasing the temperature beyond a certain limit, he made the very unexpected discovery that the positions of the optic axes were permanently altered. Thus in felspar the permanent change occurs at a low red heat, or at about 600° C. M. Des Cloizeaux exhibited this experiment in the presence of the Chemical Section of the British Association at the Meeting held in Cambridge

in 1862. This discovery leads to a conclusion of great importance to the geologist. The optic axes of a crystal of felspar serve as the index of a self-registering thermometer, and show that the crystal had never been exposed to a temperature reaching to 600° C.

Circular polarization was first discovered in quartz by Arago in 1811, and in chlorate of soda, bromate of soda, and acetate of uranium and soda, all three belonging to the cubic system, by Marbach in 1854. In 1857 M. Des Cloizeaux detected its existence in cinnabar, producing a rotation of a plane-polarized beam, sometimes to the right, sometimes to the left, to from 15 to 17 times the angle of rotation due to an equal thickness of quartz,—and also, nearly at the same time, in sulphate of strychnine with 13 combining-weights of water, a crystal belonging to the pyramidal system, the rotation, in one direction only (Herschel's right-handed rotation), the same as that of its solution, and about half as great as in an equal thickness of quartz. In February 1869 M. Des Cloizeaux discovered that benzile ($C^{14}H^{10}O^2$), a compound obtained by Laurent from the essence of bitter almonds in 1835, in crystals belonging to the rhombohedral system, produced a rotation a little greater than that of quartz. The crystals which came first into his possession were all dextrogyre; but he has lately obtained some producing a rotation in the opposite direction.

In his memoir on the Crystallization and Internal Construction of Quartz he has largely employed the method of exploration by polarized light.

During the summers of 1845 and 1846 he paid two visits to Iceland, in one of which, conjointly with M. Bunsen, he determined the temperatures, at different depths, of the great Geyser and the Strokkur, with the view of discovering the source of their heat, and the cause of the eruptions.

Any remarks on his very numerous and valuable determinations of the forms of crystalline species, as well as on the geological observations made by him in Iceland, would be obviously out of place in a summary of the researches on light and heat which render him eligible as a recipient of the Rumford Medal.

PROFESSOR MILLER,

In Monsieur Des Cloizeaux's unavoidable absence, I request you, as our Foreign Secretary, to convey to him this Medal, in recognition of the value which the Royal Society attaches to his researches.

GENTLEMEN,

If elected for the year which commences this day, and if I should be able to meet you here at your next anniversary, it will be to deliver over this Chair, doubtless to a younger, it may well be to a worthier occupant; it can hardly be to one having the welfare of the Royal Society more warmly at heart.

On the motion of Mr. Francis Galton, seconded by Colonel Smythe, it was resolved,—“ That the thanks of the Society be returned to the President for his Address, and that he be requested to allow it to be printed.”

The Statutes relating to the election of the Council and Officers having been read, and Capt. Sherard Osborn and Mr. Edward Solly having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were collected, and the following were declared duly elected as Council and Officers for the ensuing year.

President.—General Sir Edward Sabine, R.A., K.C.B., D.C.L., LL.D.

Treasurer.—William Spottiswoode, Esq., M.A.

Secretaries.— { Willim Sharpey, M.D., LL.D.
 { George Gabriel Stokes, Esq., M.A., D.C.L., LL.D.

Foreign Secretary.—Professor William Hallowes Miller, M.A., LL.D.

Other Members of the Council.—George Burrows, M.D.; Heinrich Debus, Esq., Ph.D.; Prof. Peter Martin Duncan, M.B.; Sir Philip de M. Grey Egerton, Bart.; Prof. George Carey Foster, B.A.; Francis Galton, Esq.; John Peter Gassiot, Esq., D.C.L.; Joseph Dalton Hooker, C.B., M.D.; William Huggins, Esq., D.C.L., LL.D.; Prof. George M. Humphry, M.D.; John Gwyn Jeffreys, Esq.; Sir John Lubbock, Bart.; Charles William Siemens, Esq., D.C.L.; Prof. Henry J. Stephen Smith, M.A.; Prof. John Tyndall, LL.D.; Prof. Alexander W. Williamson, Ph.D.

The thanks of the Society were voted to the Scrutators.

The following Table shows the progress and present state of the Society with respect to the number of Fellows :—

	Patron and Royal.	Foreign.	Com- pounders.	£4 yearly.	Total.
November 30, 1869.	4	49	284	260	597
Since elected		+2	+6	+11	+19
Since compounded . .			+1	—1	
Since deceased	—1	—1	—10	—7	—19
November 30, 1870.	3	50	281	263	597

1870.7

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Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55*A.* 2 *a.* 2 *r.*), £12*6* per annum.

Estate at Acton, Middlesex (34 *A.* 2 *a.* 27½ *r.*), £108 10*s.* per annum.

Pease Farm near Lewes, Sussex, rent £19 4*s.* per annum.

One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £3 per annum.

£14,000 Reduced 3 per Cent Annuities.

£29,569 15*s.* 7*d.* Consolidated Bank Annuities.

£513 8*s.* 8*d.* New 2½ per Cent. Stock Bakerian and Copley Medal Fund.

£660 Madras Guaranteed 5 per Cent. Railway Stock—Davy Medal Fund.

£708 17*s.* 1*d.* Oliveira Bequest, balance at Deposit.

Scientific Relief Fund

Investments up to July 1870, New 3 per Cent. Annuities.....	£6191 10 8
Metropolitan 3½ Consols.....	£ 100 0 0
	<u>£6291 10 8</u>

Dr.		Cr.
Balance	£	s. d.
Dividends	381	15 7
Donations	182	19 5
	115	0 0
Sale of Stock	96	2 6
	<u>674</u>	<u>17 8</u>

By Grants	£	s. d.
Stock bought	275	0 0
Balance.....	325	14 0
	174	3 6
	<u>774</u>	<u>17 8</u>

Statement of Income and Expenditure (apart from Trust Funds) during the Year ending November 30, 1870.

	£	s.	d.
Annual Subscriptions	1076	4	0
Admission Fees.....	160	0	0
Compositions	280	0	0
Rents	252	17	0
Dividends on Stock (exclusive of Trust Funds)	1012	15	0
" on Stevenson Bequest	466	1	4
Sale of Transactions, Proceedings, &c.	822	19	0
Davy Bequest (Legacy Duty repaid)	72	15	6
Oliveira Bequest (Interest)	25	12	8
Petty Repayments	1	18	0
Income available for the Year ending Nov. 30, 1870	3871	2	6
Expenditure in the Year ending Nov. 30, 1870	4108	12	2
Excess of Expenditure over Income in the Year ending Nov. 30, 1870	£182	9	8

	£	s.	d.
Salaries, Wages, and Pension	1053	19	4
The Scientific Catalogue	415	13	6
Books for the Library	159	12	5
Binding ditto	115	18	9
Printing Transactions, Part II. 1869 and Part I. 1870.....	349	10	3
Ditto Proceedings, Nos. 115-122.....	320	10	11
Ditto Miscellaneous	114	1	11
Paper for Transactions and Proceedings	226	3	0
Binding and Stitching ditto	135	19	10
Engraving and Lithography	889	19	7
Fittings, Cleaning, and Repairs	26	8	10
Miscellaneous Expenses	47	4	2
Coal, Lighting, and Gas Repairs	116	18	3
Tes Expenses	23	13	2
Fire Insurance.....	20	1	6
Estate Survey charges.....	12	19	6
Taxes.....	12	0	7
Advertising	18	8	6
Postage, Parcels, and Petty Charges.....	47	2	2
Mablethorpe Schools, Donation.....	2	2	0
	321	13	8
	24108	12	2

W. SPOTTISWOODE

*Presents received November 17, 1870.***Transactions.**

Batavia:—Bataviaasch Genootschap van Kunsten en Wetenschappen. Verhandelingen. Deel 33. 4to. *Batavia* 1868. Notulen van de Algemeene en Bestuurs-Vergaderingen. Deel 4. Afl. 2; Deel 5, 6, 7. No. 1. 8vo. *Batavia* 1867-9. Tijdschrift voor Indische Taal-, Land-, en Volkenkunde. Deel 16. Afl. 2-6; Deel 17. Afl. 1-6; Deel 18. Afl. 1. 8vo. *Batavia* 1866-68. Katalogus der Ethnologische Afdeeling van het Museum. 8vo. *Batavia* 1868. Katalogus der Numismatische Afdeeling. 8vo. *Batavia* 1869.

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Haarlem:—Hollandsche Maatschappij der Wetenschappen. Natuurkundige Verhandelingen. Deel 12, 24. 8vo. *Haarlem* 1824-44. Tweede Verzameling. Deel 5. Stuk 2; Deel 13, 20. C. H. Hoffmann u. H. Weyenbergh, Die Osteologie und Myologie von Sciurus Vulgaris. W. F. R. Suringar, Algæ Japonicæ. 4to. *Haarlem* 1850-70. Archives Néerlandaises des Sciences Exactes et Naturelles. Tome V. liv. 1-3. 8vo. *La Haye* 1870.

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London:—British Museum. Hand List of Genera and Species of Birds, by G. R. Gray. Part 2. Supplement to the Catalogue of Shield Reptiles, by J. E. Gray. Part I. 4to. *London* 1870. Catalogue of Lithophytes, or Stony Corals, by J. E. Gray. 8vo. *London* 1870. Catalogue of the Fishes, by A. Günther. Vol. VIII. 8vo. *London* 1870. Index and Guide to the Collection of Minerals. Guide to the Exhibition Rooms of the Departments of Natural History and Antiquities. Guide to the Autograph Letters, &c. 8vo. *London* 1870. Catalogue of Prints and Drawings. Division 1. Satires. Vol. I. 1320-1689. 8vo. *London* 1870.

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Linnæan Society. Transactions. Vol. XXVI. Part 4; Vol. XXVII.

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- Parts 1 & 2. 4to. *London* 1870. Journal. Zoology, Vol. XI. No. 49. Botany, Vol. XI. Nos. 54, 55. 8vo. *London* 1870. Proceedings, Session 1869-70, pp. xxxiii-cxx. Additions to the Library. List. 8vo. The Society.
- Royal Agricultural Society. Journal. Second Series. Vol. VI. Part 2. 8vo. *London* 1870. The Society.
- Royal Institution. Proceedings. Vol. V. Part 7; Vol. VI. Parts 1 & 2. 8vo. *London* 1869-70. List of the Members, &c. The Institution.
- Royal Medical and Chirurgical Society. Medico-Chirurgical Transactions. Vol. LIII. Proceedings. Vol. VI. No. 6. 8vo. *London* 1870. The Society.
- Science and Art Department of the Committee of Council on Education. The First Proofs of the Universal Catalogue of Books on Art. Vol. II. 4to. *London* 1870. The Committee of Council.
- Society of Antiquaries. Archæologia. Vol. XLII. Part 2. 4to. *London* 1870. Proceedings. Second Series. Vol. IV. No. 8. 8vo. *London* 1870. The Society.
- New York:—American Geographical and Statistical Society. Journal. Vol. II. Part 2. 8vo. *New York* 1870. The Society.
- Newcastle:—Natural-History Transactions of Northumberland and Durham. Vol. III. Part 2. 8vo. *Newcastle-upon-Tyne* 1870. Tyneside Naturalists' Field Club.
- Paris:—École Impériale Polytechnique. Journal. Tome XXVI. (Cahier 43.) 4to. *Paris* 1870. The School.
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- Salem (Mass.):—Essex Institute. Proceedings. Vol. V. Nos. 2, 3, 5, 6, 7; Vol. VI. Part 1. Bulletin. Vol. I. No. 1-12. 8vo. *Salem* 1867-70. The Institute.
- Peabody Academy of Science. First Annual Report of the Trustees, January 1869. 8vo. *Salem* 1869. The American Naturalist. Vol. III. Nos. 1-12; Vol. IV. Nos. 1, 2. 8vo. *Salem* 1869-70. The Academy.
- Warwick:—Warwickshire Natural-History and Archæological Society. Annual Report. 1, 2, 5-16, 18, 25-34. 8vo. *Warwick* 1837-70. Proceedings of the Warwickshire Naturalists' and Archæologists' Field Club. 1865, 1868. 8vo. *Warwick*. The Society.
- Washington:—Smithsonian Institution. Smithsonian Contributions to Knowledge. Vol. XVI. 4to. *Washington* 1870. Smithsonian Miscellaneous Collections. Vol. VIII., IX. 8vo. *Washington* 1869. Annual Report of the Board of Regents for the year 1868. 8vo. *Washington* 1869. The Institution.

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Australian Medical Journal. No. 108-112. 8vo. *Melbourne* 1870.
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Zeitschrift für Chemie. Jahrgang 9-12. 8vo. *Leipzig* 1866-69.
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Dr. F. Brunnnow.
Leyton :—Astronomical Observations taken during the years 1865-69 at the Private Observatory of J. G. Barclay, Esq. Vol. II. 4to. *London* 1870.
J. G. Barclay, Esq.
London :—Meteorological Office. Quarterly Weather Report. Part 1. Jan.-March 1869. 4to. *London* 1870. Weather Reports. Jan. 1 to June 30, 1870. folio. *London*. Report of the Meteorological Committee of the Royal Society for the year ending 31st December, 1869. 8vo. *London* 1870. The Office.
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Dealongchamps (Eugène E.-) Notes Paléontologiques. Vol. I. 8vo. *Caen* 1863-69. The Author.
Duhamel (J. M. C.) Des Méthodes dans les Sciences de Raisonnement. Partie 4. 8vo. *Paris* 1870. The Author.
Harris (George) The Theory of the Arts, or Art in relation to Nature, Civilization, and Man. 2 vols. 8vo. *London* 1869. The Author.
Holland (T. J.) and H. Hozier Record of the Expedition to Abyssinia. 2 vols. 4to, and case of Maps. *London* 1870.
The Secretary of State for War.
Jardine (Sir Wm.), F.R.S. Natural History and Illustrations of the Scottish Salmonidæ. 12 plates in portfolio.
General Sir Edward Sabine, K.C.B., P.R.S.
Lubbock (Sir John), F.R.S. The Origin of Civilization and the Primitive Condition of Man. 8vo. *London* 1870. The Author.

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Poncelet (J.V.), For. Mem. R.S. Introduction à la Mécanique Industrielle physique ou expérimentale; troisième édition, par X. Kretz. 8vo. *Paris* 1870.

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Secchi (A.), For. Mem. R.S. Le Soleil. 8vo. *Paris* 1870.

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Greenwich:—Royal Observatory. Astronomical and Magnetical and Meteorological Observations made in the year 1868. 4to. *London* 1870.

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Melbourne:—Observatory. Results of Astronomical Observations made in the year 1866, 1867, and 1868. 8vo. *Melbourne* 1869.

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Tables of Harmonia, by E. Schubert. 4to. *Washington* 1869.

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Helmholtz (H.), For. Mem. R. S. Die Lehre von den Tonempfindungen. Dritte umgearbeitete Ausgabe. 8vo. *Braunschweig* 1870.

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Jones (Joseph) Suggestions on Medical Education. Introductory Lecture in the Medical College of Georgia. 8vo. *Augusta* 1860. Agricultural Resources of Georgia. Address before the Cotton Planters' Convention of Georgia at Macon. 8vo. *Augusta* 1861. Researches upon "Spurious Vaccination." 8vo. *Nashville* 1867. Chemical Analysis of Louisiana Rock-salt. 8vo. *New Orleans* 1869. Mollities Ossium. 8vo. *Philadelphia* 1869. Observations and Researches on Albinism in the Negro Race. 8vo. *Philadelphia* 1869.

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Walenn (W. H.) Patents for Inventions. Abridgments of Specifications relating to Aëronautics, A.D. 1815-1866. 8vo. *London* 1869.

The Compiler.

A.—Account* of the appropriation of the sum of £1000 annually voted by Parliament to the Royal Society (the Government Grant), to be employed in aiding the advancement of Science. The present statement is in continuation of that already given up to April 5, 1855, in Volume VII. pp. 512-522.

1855.

- | | |
|--|-----|
| 1. To the Rev. F. Bashforth, for inquiries concerning Capillary Attraction..... | £50 |
| 2. To Dr. Miller, on behalf of the Kew Observatory, for the construction and verification of Standard Meteorological Instruments . | 100 |

* By resolution of the Council of Dec. 15, 1870, it has been ordered that accounts of the Expenditure of the Government Grant, and of the sums granted from the Donation Fund, shall be published annually in the Proceedings, with the Report of the Anniversary Meeting.

3. To Dr. Salter, for inquiries in Experimental Physiology	£50
4. To Dr. Frankland, for continuation of his researches on Organo-Metallic Bodies	100
5. To Mr. Fairbairn, for experiments on the Explosions of Steam-boilers	300
6. To Professor E. Hodgkinson, for continuing his experiments on the Strength of Materials	100
7. To Dr. Carpenter, for researches in Marine Natural History .	50
8. To Mr. H. F. Baxter, for researches in Electro-Physiology..	30

1856.

1. To Dr. Gladstone, for researches on Chemical Affinity	50
2. To Dr. Tyndall, for continuing researches in Magnetism	100
3. To Mr. Cooper, for expense of printing the fourth and last volume of his Catalogue of Ecliptic Stars	140
4. To Dr. Harley, for continuing his researches on the Chemistry of Respiration	50
5. To Mr. Greville Williams, for an investigation of the Products of Distillation of Coal at low temperatures.	50
6. To Mr. Lockhart Clarke, for investigations into the structure of the <i>Medulla oblongata</i> and <i>Pons Varolii</i> of Man, and some of the Vertebrata.	30
7. To Dr. Brown-Séguard, for researches on the vital properties of Muscles, Nerves, and the Spinal Chord	60
8. To Professor William Thomson, for continuing his Electrical researches.	100
9. To Professor Eaton Hodgkinson, for continuing his experiments on the Strength of Materials	100
10. To Dr. Carpenter, for the prosecution of his researches on Marine Natural History	50
11. To Professor Owen, for obtaining drawings of the <i>Scelido-therium leptcephalum</i> and other extinct animals	50

1857.

1. To Dr. Waller, for prosecuting his investigations on the Nervous System	50
2. To Mr. Hopkins, for continuation of his researches on the effect of Pressure on the Melting-point of Solids.	150
3. To Dr. Roscoe, for prosecuting Photo-chemical researches, in association with Professor Bunsen	100
4. To Professor W. Thomson, for Electrical researches.	50
5. To Mr. Beckles, for further prosecuting the search for Fossil Remains of Mammalia in the Purbeck Strata	150
6. To Dr. Debus, for prosecuting investigations on the action of Nitric Acid on Alcohol.	50

Appropriation of the Government Grant. 137

7. To Mr. Greville Williams, for continuing his researches on the products of distillation of Boghead Coal.....	£50
8. To Mr. Henry Lee, for the investigation of Morbid Processes, arising from affections of the Blood-vessels and their contents....	25
9. To Dr. Brown-Séquard, for continuing his researches on the vital properties of Nerves and Muscles, and on Animal Heat....	100
10. To the Committee of the Kew Observatory, for completing new Photographic Magnetic Instruments, now in process of construction at the Observatory.....	150

1858.

1. To Dr. Edward Smith, for prosecuting his researches on Respiration.....	100
2. To Mr. Mallet, for conducting investigations on the spot, of the phenomena resulting from the recent Earthquakes in the Neapolitan Territory.....	150
3. To Mr. Greville Williams, for researches on the constitution of the Oil of Rue, and on the influence of temperature and pressure on the Densities of Vapours.....	100
4. To Mr. Matthiessen, for researches on the law by which Alloys conduct Electricity.....	50
5. To Mr. Gore, for an inquiry into the Molecular states of Metals.....	50
6. To Mr. Wanklyn, for researches on a new series of bodies containing Potassium and Sodium.....	50
7. To Professor Tyndall, for continuing his researches on Glaciers.....	15
8. To Dr. Frankland, for continuing his researches on Organo-metallic Bodies.....	100
9. To Mr. Hopkins, for continuing his researches on the Effect of Pressure on the Melting-points of Solids.....	150
10. To Dr. Pavy, for researches on the Physiology of the Liver.	50
11. To Mr. Matthiessen, for researches on the Action of Nitrous Acid on Natural Organic Bases.....	50

1859.

1. To Mr. Mallet, for obtaining photographs of the Earthquake phenomena in the Neapolitan Territory.....	50
2. To Professor Owen, for procuring drawings and other illustrations of new and undescribed subjects of Comparative Anatomy and Palæontology.....	100
3. To Professor William Thomson, for defraying expenses already incurred, and continuing his researches on the Electro-dynamic Qualities of Metals.....	100

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|---|------|
| 4. To the Brixham Cave Committee, for continuing the Excavations | £100 |
| 5. To Dr. Tyndall, for investigating the Minimum Temperature both of the Air and of the Ice at various elevations on Mont Blanc . | 100 |

1860.

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|---|-------------|
| 1. To Mr. J. P. Joule, for experiments on Surface Condensation | 50 |
| 2. To Dr. Pavy, for continuing his Chemico-Physiological researches | 50 |
| 3. To Dr. Maxwell Simpson, for continuing his researches on Glycol | 50 |
| 4. To Mr. W. Fairbairn, for researches on the Density of Steam | 60 |
| 5. To Professor William Thomson, as an Electrical Committee of the British Association, for the construction of Self-recording Electrical Instruments | 100 |
| 6. To Mr. C. Greville Williams, for continuing his researches on Euodic Aldehyde, &c. | 100 |
| 7. To the Rev. Dr. Robinson, for the completion of the Armagh Catalogue of Stars | 56 15s. 8d. |
| 8. To Mr. A. Matthiessen, for researches on the conducting-powers for Heat and Electricity, and on the coefficients of expansion, of Metals and Alloys | 50 |
| 9. To Mr. G. Gore, for continuing his researches on Electro-deposited Antimony, and for further examining the movements of Liquid Metals and Electrolytes in the Voltaic Circuit | 50 |
| 10. To Mr. T. Rupert Jones and Mr. W. K. Parker, for the completion and publication of Drawings of the Foraminifera | 100 |
| 11. To Mr. Warren De La Rue, for expenses of conveying the Kew Photoheliograph to Spain, and travelling-expenses of two assistants, to obtain Photographs of the Solar Eclipse of 18th July, 1860 | 150 |

1861.

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|---|-----|
| 1. To Dr. Pavy, for continuing his researches on the Physiology of the Liver | 100 |
| 2. To Dr. Joule and Professor Thomson, for completing their researches on the Thermal effects of Fluids in Motion | 150 |
| 3. To Professor Beale, for the Augmentation of the Powers of Microscopes with reference to their employment in Physiological researches | 50 |
| 4. To Mr. Maxwell Simpson, for researches on the Cyanides of the Diatomic and Triatomic Radicals | 50 |
| 5. To Dr. Frankland, for examining the action of Zinc-ethyl and analogous bodies on Silicic, Carbonic, and Oxalic Ethers | 100 |

Appropriation of the Government Grant.

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6. To Mr. Mallet, for the publication of his Earthquake Report (to be applied to the illustrations)	£300
7. To Mr. Balfour Stewart, for researches on the Diathermancy of Bodies	50
8. To Mr. Balfour Stewart, for experiments to determine accurately certain Melting-points, to be used as standard points for high and low temperatures	150
9. To Mr. Hopkins, for continuing his researches on the Specific Heat, Expansibility, and Temperatures of Fusion under pressure, of certain substances	100
10. To Professor Tyndall, for investigating the relations of Gases and Vapours to Radiant Heat	200
11. To Mr. Cayley, for certain Analytical Calculations	20

1862.

1. To Dr. J. D. Hooker, for procuring drawings to illustrate the description of a new and remarkable plant discovered in Angola by Dr. Wellwitsch	50
2. To Mr. E. W. Binney, for defraying the expenses attending the investigation of structural specimens of Coal Fossils of remarkable interest and perfection	50
3. To Dr. E. Smith, for researches with a view to determine the relations of Nitrogen in the food and egesta of patients afflicted with Fever and Phthisis	50
4. To Professor J. Phillips, for defraying the cost of maintaining, and the incidental expenses of working, a telescope at Oxford for the special examination of the Moon's Surface	100
5. To Professor W. Thomson, for experimental researches, (1) on Contact Electricity, (2) on the absolute conducting-powers of different substances for Heat	50
6. To Professor W. Thomson, for observation of Atmospheric Electricity: for instruments to be entrusted to Mr. B. Stewart, Kew	14 10s.
7. To Mr. G. Gore, for continuing his experiments on the Electrolytic Vibrations of Mercury, and other allied matters	50

1863.

1. To Dr. Waller, for experiments on a means of avoiding the danger attending the administration of the Vapour of Ether or Chloroform, and for investigating Lead-poisoning	50
2. To Professor Maxwell Simpson, for further researches on Cyanides of the di- and triatomic Radicals	50
3. To Mr. Warren De La Rue, for the continuation at Kew for one year of the observations with the Photoheliograph	200
4. To Professor Selwyn, for obtaining Autographs of the Sun . .	50

5. To Mr. Mallet, for determining the Temperature of Volcanos	£100
6. To Dr. Matthiessen,—I. for a research into the thermo-electric behaviour of Alloys, to be carried out in conjunction with Mr. Matthews	25
7. II. For a research into the Expansion by Heat of Metals and Alloys, to be carried out in conjunction with Dr. Dupré	25
8. III. For a research into the Conduction of Heat by Metals and Alloys, to be carried out in conjunction with Dr. Schunck	25
9. To Mr. Crookes, for continuing his researches on Thallium	200
10. To Dr. Frankland, for continuing his researches on Organo-metallic Bodies	100
11. To Professor William Thomson, for procuring Instruments for the observation of Atmospheric Electricity in Nova Scotia	20
12. To Mr. W. R. Birt, for the purchase of a 4-inch Object-glass for observing the Physical Features of the Moon	60
13. To Dr. W. A. Miller, for Apparatus for the investigation of the Spectrum	150

1864.

1. To Mr. R. C. Carrington, towards defraying the cost of Printing and Publishing his Observations of Solar Spots	250
2. To Mr. T. A. Malone, for investigating the nature of the Action and Results obtained in the Daguerreotype and Photographic Processes	50
3. To the Rev. Dr. Robinson, for reducing a series of Anemometric Registrations	125
4. To Mr. Gassiot (on behalf of the Kew Committee), for obtaining Copies of Magnetic Curves taken at Kew, for distribution	50
Ditto, ditto, for procuring a self-recording Anemometer for observations to be taken at the Island of Ascension	69 1s.
5. To Dr. Pavy, for Physiological Researches into the Stomach and Liver	50
6. To Mr. F. Jenkin (on behalf of a Committee of the British Association), to supplement Grants voted by the British Association for the construction of four Standard Electrical Instruments for the Observatory at Kew	50
7. To Dr. Matthiessen, for further researches into Narcotine	30
8. To Mr. Schorlemmer, for investigations into the whole series of the so-called Alcohol Radicals	30
9. To Mr. De La Rue, for continuation of Photoheliography at Kew till February 1865	150
10. To Dr. Richardson, for an Inquiry into the best means of restoring suspended Animation	50
11. To Professor Tyndall, for researches on Radiant Heat as applied to Molecular Physics	100

Appropriation of the Government Grant.

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12. To the Rev. H. Tristram, for expenses of a Botanical Collector in the Expedition to the shores of the Dead Sea and adjacent country	£50
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1865.

1. To Mr. De La Rue, for continuing the observations with the Kew Photoheliograph to November 1865	100
2. To Mr. Balfour Stewart, for defraying the cost of obtaining and distributing Magnetic Curves	100
Ditto, for repair of Pendulums, and fitting a room at Kew for Base Observations	200
3. To Dr. Frankland, for continuing his researches on the Synthesis of Organic Compounds	100
4. To Dr. Maxwell Simpson, for continuing his researches on the Polyatomic Radicals	50
5. To Mr. R. C. Carrington, for reducing Weisse's and Oeltzen's Catalogue of Stars	90
6. To the Rev. C. Pritchard, for arranging and computing Tables for facilitating the construction of Aplanatic Object-glasses .	60
7. To the Rev. Dr. Robinson (on behalf of a Committee of the British Association), for experiments on Submarine Fog-signals. . .	100
8. To Dr. Falconer, in furtherance of a determination by Levelling of the exact depression of the Dead Sea (the amount to be placed at the disposal of Col. Sir Henry James)	100
9. To Professor Stokes, for determining, by means of pendulums, the Index of Friction of Gases and Vapours	100
10. To Mr. F. Galton, for an Apparatus for verifying Sextants in connexion with Kew Observatory	80
11. To Dr. Richardson, for continuing his Inquiries into the means of restoring suspended Animation.	50
12. To Mr. G. J. Symons, for the salary of an Assistant to be employed in collecting and classifying Rainfall Statistics	50
13. To Mr. De La Rue, for continuation at Kew Observatory of Observations on Sun-spots	150
14. To the Rev. G. C. Hodgkinson, for Actinometrical Instruments and Observations	30

1866.

1. To Professor Stokes, for continuing his experiments on the Index of Friction of Gases and Vapours.	75
2. To Sir Henry James, for half the excess of the expense of levelling to determine the exact depression of the Dead Sea, over the sums granted by the Royal Society and Royal Geographical Society for the purpose	7 7s. 9d.

3. To Mr. F. Jenkin, for the construction of a Standard Electrodynamometer	£75
4. Ditto, for the construction of a Standard Condenser, or Leyden-jar	75
5. To Mr. B. Stewart, for determining the Origin of the Heat observed in a Revolving Disk	50
6. Ditto, for determining the Rate and Length of Kater's Invariable Pendulum	100
7. To the Rev. A. Weld, for half the expense of establishing a Magnetic Observatory at Stonyhurst	225
8. To Mr. G. Bentham and others, for preparing a Catalogue of all described Phænogamous Plants	50
9. To Professor Huxley, towards defraying the cost of coloured Plates for a work, by Mr. Parker, on the Sternum and Shoulder-girdle of the Vertebrata	100

1867.

1. To Mr. De La Rue, for working the Kew Photoheliograph during the current year	200
2. To Mr. J. N. Lockyer, for the purchase of a large Spectroscope, and of fitting the same to his Telescope, to be employed in Spectroscopic Observations of the Sun	40
3. To Dr. Gamgee, for investigating the Action of Carbonic Oxide and other Poisonous Agents upon Blood	50
4. To the Rev. G. C. Hodgkinson, for continuing his Actinometrical researches	30
5. To Dr. Frankland, for continuing his Synthetical researches on Ethers	100
6. To Mr. Breen, for the Correction of the Elements of the Orbits of Jupiter and Saturn	50
7. To Dr. Carpenter, for defraying the expenses incurred in the prosecution of his researches in Marine Zoology	100
8. To Mr. De La Rue, for reducing Schwabe's Observations of Sun-spots	60
9. To Mr. Scott (as Secretary of the Greenland Committee of the British Association), for the Exploration of the Tertiary Plant Beds of North Greenland	200
10. To Mr. B. Stewart, for an Apparatus for verifying Sextants in connexion with Kew Observatory (in addition to the former grant)	40
11. To Dr. Frankland, for investigating the Luminosity of various Flames under Pressure	25
12. To Dr. Bigsby, for assisting in the publication of his work entitled "Thesaurus Siluricus," it being understood that 50 copies	

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would be placed at the disposal of the Royal Society, and 50 copies at the disposal of the Geological Society for distribution £100

13. To Prof. Roscoe, for continuing his experiments on the Chemical Intensity of total Daylight. 100

1868.

1. To Mr. Herbert Jenner Fust, for assistance towards the publication of a paper "On the Distribution of Lepidoptera in Great Britain and Ireland" 25

2. To Dr. Maxwell Simpson, for continuing his researches on the Cyanides of the Alcohol Radicals and their derivatives 100

3. To Dr. Matthiessen and Mr. Hocken, for a research into the Conducting-power of Liquids for Electricity 40

4. To Dr. Matthiessen and Mr. R. Sabine, for Apparatus for prosecuting a research into the determination of High Temperatures by means of a new Pyrometer 50

5. To Mr. R. Scott (on behalf of the Greenland Committee of the British Association), for defraying some additional expenses connected with the Collection of Greenland Fossils 25

6. To Mr. G. J. Symons, for a continuation of Rainfall Observations 100

7. To Mr. C. Brooke, for construction of a Spectroscope with six Rock-salt Prisms 60

8. To Mr. G. Bentham, for bringing up the Catalogue of Phænogamous Plants at the Kew Herbarium to a point from which it can be continued by the ordinary Staff 25

9. To Mr. B. Stewart, for continuing his experiments on a Rotating Disk 60

10. To Mr. De La Rue, for continuing the Kew Photoheliographic Observations 200

11. To the Treasurer of the Royal Society, for defraying the expense of Instruments sent to India 284 18s. 6d.

12. To Mr. W. Carruthers, for Illustrations of British Fossil Cycadeæ 50

1869.

1. To the Rev. S. Haughton, for investigation of the Granites of Scotland 50

2. To Mr. G. J. Symons, for comparison of the effect of different forms of Thermometer Stand 15

3. To Mr. F. Guthrie, for prosecuting experiments on the Thermal Resistance of Liquids 50

4. To Mr. G. Gore, for continuing his researches on the Fluorides 150

5. To Mr. E. T. Chapman, for a research on the Physical Properties of Organic Bodies 50

6. To Dr. Sanderson, for further researches on Respiration and the Circulation of the Blood 50

Appropriation of the Government Grant.

7. To Mr. F. Galton, for construction of his Anemometer	£15
8. To Mr. De La Rue, for continuing the Kew Photoheliographic Observations	200
9. To Mr. J. N. Lockyer, for a continuation of his Spectroscopic Observations on the Sun	60
10. To Prof. Cayley, Mr. Clerk Maxwell, Prof. Sylvester, for the construction of Models of certain Geometrical Surfaces	40
11. To Dr. Carpenter, for the further prosecution of researches into the Temperature and Zoology of the Deep Sea	200
12. To Mr. Dupré and Mr. F. J. Page, for continuing their investigations on the Specific Heat and other Physical Characters of Aqueous Mixtures and Solutions	70

1870.

1. To Mr. G. J. Symons, for comparison of the effect of different forms of Thermometer Stand, in addition to the former Grant	30
2. To Professor Tait, for an Inquiry into Thermal Conductivity .	50
3. To Mr. R. Field, for experiments to determine the amount of evaporation from a Water-surface	30
4. To Mr. A. Dupré, for continuing investigations on the Specific Heat and other Physical Characters of Aqueous Mixtures and Solutions	30
5. To Mr. F. Galton, for excess of cost of his Anemometer . . 7 19s. 6d.	
6. To Mr. De La Rue, for continuing the Kew Photoheliographic Observations	200
7. To Mr. Schorlemmer, for continuing his researches on the Hydrocarbons	30
8. To Dr. Carpenter, for the Scientific Expenses of a contemplated Dredging Expedition	100

Total Grants and Appropriations.

GRANTS.				APPROPRIATIONS.			
Repayments :—							
	£	s.	d.		£	s.	d.
					13970	12	5
W. De La Rue	269	16	0	Overdrawn in 1854 .	71	10	8
L. Horner . . .	20	0	0	Petty charges	0	14	2
Prof. Stokes . .	175	0	0				
Sundry	0	5	0		£14042	17	3
W. Hopkins . .	100	0	0	Repaid to the Trea- }	1000	0	0
R. H. Scott . .	7	5	3	sury }			
	572	6	3	Balance Nov. 30, 1870	1627	4	2
Interest	97	15	2				
	£16670	1	5		£16670	1	5

WILLIAM SPOTTISWOODE,
V.-P. and Treasurer R. S.

B.—Account of Sums granted from the Donation Fund in 1870.

1. Mr. Warren De La Rue, for the enlarging of certain Solar Negatives obtained at the Kew Observatory	£11 11s.
2. Dr. Carpenter, for the purchase of a specimen of <i>Pentacrinus Caput Medusæ</i>	25
3. Mr. Edward Waller, for the exploration of the Sea-bed on the North-western coast of Ireland by means of the Dredge, in continuation of the researches made last year in H.M.S. 'Porcupine'	100
4. Mr. J. P. Gassiot, to defray expense of making six prints from the Negatives of Sun-pictures taken at the Kew Observatory during the years 1862–72, with the view of presenting them to the Royal Society, the Royal Astronomical Society, the Imperial Academy of Sciences of Paris and of St. Petersburg, the Royal Academy of Sciences, Berlin, and the Smithsonian Institution, Washington (£120 in two payments)	60
5. Mr. William Saville Kent, in aid of a Zoological Dredging-expedition in a private yacht off the west coast of Spain and Portugal.	50
6. Dr. Bastian, for carrying on certain experiments with a Digester capable of sustaining high Temperatures.	10
	<hr/> £256 11s.

December 8, 1870.

General Sir EDWARD SABINE, K.C.B., President, in the Chair.

The President announced that he had appointed the following Members of the Council to be Vice-Presidents :—

The Treasurer.
 Sir Philip Grey-Egerton, Bart.
 Mr. Francis Galton.
 Dr. Huggins.

The following communication was read :—

“Report on Deep-sea Researches carried on during the Months of July, August, and September 1870, in H.M. Surveying-ship ‘Porcupine.’” By W. B. CARPENTER, M.D., F.R.S., and J. GWYN JEFFREYS, F.R.S. Received December 2, 1870.

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[At the time at which this Report was presented, it was hoped that the General Summary of Results, of which it then consisted, could be amplified by the insertion of the requisite details, within the time at which the publication of the Proceedings would be due. It has been found, however, that in working out these details so many new points arose suggestive of further inquiries (especially requiring careful comparison of Temperature observations) that, though devoting to them all the time he could command, the Member of the Expedition who is responsible for the whole, save the Narrative of the First Cruise, has found himself unable to complete his portion of the Report at an earlier date. Whilst expressing his regret for the delay which has hence arisen, he ventures to hope that some compensation for it will be found in the greater completeness which the Report now possesses; especially in that Section of it which treats of the Causal Relation between the double currents of the Straits of Gibraltar and Baltic Sound, and the General *vertical* Oceanic Circulation.—W. B. C.]

INTRODUCTION.

PRELIMINARY PROCEEDINGS.

The following Extracts from the Minutes of the Council of the Royal Society set forth the origin of the ‘Porcupine’ Expedition of 1870, and the objects which it was designed to carry out.

March 24, 1870.

A Letter was read from Dr. Carpenter, addressed to the President, suggesting that an Exploration of the Deep Sea, such as was carried out during 1868 and 1869 in the regions to the North of the British Islands, should now be extended to the South of Europe and the Mediterranean,

and that the Council of the Royal Society should recommend such an undertaking to the favourable consideration of the Admiralty, with a view to obtain the assistance of Her Majesty's Government as on the previous occasions.

Resolved,—That a Committee, consisting of the President and Officers, with the Hydrographer, Mr. Gwyn Jeffreys, Mr. Siemens, Professor Tyndall, and Dr. Carpenter, with power to add to their number, be appointed, to consider the expediency of adopting the proposal of Dr. Carpenter, and the plan to be followed in carrying it out, as well as the instruments and other appliances that would be required, and to report their opinion thereon to the Council; but with power previously to communicate to the Admiralty a draft of such report as they may agree upon, if it shall appear to them expedient to do so in order to save time.

April 28, 1870.

Read the following Report :—

“ The Committee appointed on the 24th of March to consider a proposal for a further Exploration of the Deep Sea during the ensuing summer, as well as the scientific preparations which would be required for a new expedition, beg leave to report as follows :—

“ The general course proposed to be followed, and the chief objects expected to be attained in a new expedition, are pointed out in the following extract from the letter of Dr. Carpenter, read to the Council on the 24th ult., which was referred to the Committee.

“ ‘ The plan which has been marked out between my Colleagues in last year's work and myself is as follows :—

“ ‘ Having reason to hope that the ‘ Porcupine ’ may be spared towards the end of June, we propose that she should start early in July, and proceed in a S.W. direction towards the furthest point to which our survey was carried last year; carefully exploring the bottom in depths of 400 to 800 fathoms, on which, as experience has shown us, the most interesting collections are to be made; but also obtaining a few casts of the Dredge with Temperature-soundings at greater depths, as opportunities may occur.

“ ‘ The course should then be nearly due South, in a direction of general parallelism with the coast of France, Spain, and Portugal, keeping generally within the depths just mentioned, but occasionally stretching westwards into yet deeper waters. From what has been already done in about 400 fathoms' water off the coast of Portugal, there is no doubt that the ground is there exceedingly rich. When approaching the Straits of Gibraltar, the survey, both Physical and Zoological, should be carried out with great care and minuteness; in order that the important problem as to the currents between the Mediterranean and Atlantic Seas, and the relation of the Mediterranean Fauna to that of the Atlantic (on which Mr. Gwyn Jeffreys

is of opinion that the results of our last year's work throw an entirely new light), may be cleared up.

“ ‘ Mr. Gwyn Jeffreys is prepared to undertake the scientific charge of this part of the expedition ; and if Prof. Wyville Thomson should not be able to accompany him, it will not be difficult to find him a suitable assistant.

“ ‘ The ship would probably reach Gibraltar early in August, and there I should be myself prepared to join her, in place of Mr. Jeffreys, with one of my sons as an assistant. We should propose first to complete the survey of the Straits of Gibraltar, if that should not have been fully accomplished previously ; and then to proceed eastwards along the Mediterranean, making stretches between the coasts of Europe and Africa, so as to carry out as complete a survey, Physical and Zoological, of that part of the Mediterranean basin as time may permit. Malta would probably be our extreme point ; and this we should reckon to reach about the middle of September.

“ ‘ It is well known that there are questions of great Geological interest connected with the present distribution of Animal life in this area ; and we have great reason to believe that we shall here find at considerable depths a large number of Tertiary species which have been supposed to be extinct. And in regard to the Physics of the Mediterranean, it appears, from all that we have been able to learn, that very little is certainly known. The Temperature and Density of the water, at different depths, in a basin so remarkably cut off from the great ocean, and having a continual influx from it, form a most interesting subject of inquiry, to which we shall be glad to give our best attention, if the means are placed within our reach.’

“ Considering the success of the two previous Expeditions, and especially that of the ‘ Porcupine ’ last year, the Committee are persuaded that no less important acquisitions for the furtherance of scientific knowledge would be gained by the renewed exploration as now proposed ; and they accordingly recommend that a representation to that effect be made to the Admiralty, with a view to obtain the aid of Her Majesty’s Government as on the previous occasions.

“ The Committee approve of a proposal made by Mr. Gwyn Jeffreys to accept the services of Mr. Lindahl, of Lund, in the expedition as unpaid Assistant Naturalist.

“ As regards scientific instruments, the Committee have to report that those employed in last year’s voyage will be again available for use ; and Mr. Siemens hopes to render his electro-thermal indicator of more easy employment on ship-board.

“ The Committee, having learned that Dr. Frankland has contrived an apparatus for bringing up the deep-sea water charged with its gaseous contents, have resolved to add his name to their number ; and they request leave to meet again in order to complete the arrangements and make a final report to the Council.”

Resolved,—That the Report now read be received and adopted, and that the Committee be requested to continue their Meetings and report again on the arrangements when finally decided on.

Resolved,—That the following draft of a Letter to be addressed to the Secretary of the Admiralty be approved, viz. :—

“SIR,—I am directed by the President and Council of the Royal Society to acquaint you, for the information of the Lords Commissioners of the Admiralty, that, considering the important scientific results of the Physical and Zoological Exploration of the Deep Sea carried on in 1868 and 1869 through the aid of Her Majesty’s Government, they deem it highly desirable that the investigation should be renewed during the ensuing summer, and extended over a new area.

“The course which it would be proposed to follow in a new Expedition, the principal objects to be attained, and the general plan of operations, are sketched out in the inclosed extract from a Letter addressed to the President by Dr. Carpenter, and have in all points been approved by the Council.

“The President and Council would therefore earnestly recommend such an undertaking to the favourable consideration of My Lords, with the view of obtaining the assistance of Her Majesty’s Government so liberally accorded and effectively rendered on the previous occasions.

“The scientific conduct of the expedition would, as in the last year, be shared by Dr. Carpenter, Professor Wyville Thomson, provided that gentleman is able to undertake the duty, and Mr. Gwyn Jeffreys. It is also proposed that Mr. Lindahl, a young Swedish gentleman accustomed to marine researches, should accompany the expedition as Assistant Naturalist.

“I have to add that whatever appertains to the strictly Scientific equipment of the Expedition will, as formerly, be at the charge of the Royal Society.

“W. SHARPEY, *Secretary.*”

A sum of £100 from the Government Grant was assigned for the Scientific purposes of the Expedition.

May 19, 1870.

Read the following Letter from the Admiralty :—

“Admiralty, 10 May, 1870.

“SIR,—Having laid before My Lords Commissioners of the Admiralty your letter of 2nd inst., requesting that further researches may be made of the deep sea, I am commanded by their Lordships to acquaint you that they will spare Her Majesty’s Steam-vessel ‘Porcupine’ for this service, and that the Treasury have been requested, as on the former occasion, to

defray the expense of the messing of the scientific gentlemen composing the Expedition.

“ I am, Sir,

“ Your obedient Servant,

“ *To W. Sharpey, Esq., M.D.,
Secretary of the Royal Society,
Burlington House.*”

“ VERNON LUSHINGTON.”

EQUIPMENT.

1. The equipment of the ‘ Porcupine ’ for the previous Expedition had been found so complete and satisfactory that nothing more was considered necessary to prepare her for the work of the present season than the overhauling of her gear, and the manufacture of new dredges, sieves, and other apparatus, on the patterns of those which had already proved most serviceable. We had the advantage of the same excellent Commander, now promoted to the rank of Staff-Captain, with his able staff of Officers ; and we would take this opportunity of again expressing our deep sense of obligation to them all for their hearty co-operation in our scientific work, and for the unvarying personal kindness by which our voyage was rendered a most agreeable one. A considerable part of the Crew, also, consisted of the same steady and experienced men. The Meteorological Department supplied eight of the protected Miller-Casella Thermometers, including the two with the performance of which we had been so thoroughly satisfied last year ; and we usually employed one of these in conjunction with one that had not been used in the previous Expedition.

2. At the request of the Committee, Mr. Siemens undertook to devise an Apparatus for testing the depth of Sea-water to which Light, or at least the Actinic rays, can penetrate. The foundation of the apparatus which he constructed for this purpose is a horizontal wheel with three radii, each of them carrying a glass tube in which a piece of sensitized paper is sealed up. The rotation of this wheel round a vertical axis brings each of the tubes in succession out of a dark chamber in which it ordinarily lies, exposes it to light in an uncovered space, and then carries it into darkness again. This movement is produced by a spring ; but it is regulated by a detent that projects from the keeper of an Electro-magnet, which is made and unmade by the completion or breaking of a circuit that connects it with a Galvanic battery. When the magnet is made, it lifts the keeper with its projecting detent ; and this allows the wheel to be carried by the spring through one-sixth of its rotation, whereby the first of the tubes is brought out into the open space. There it remains until the circuit is broken, whereby the magnet is unmade ; the keeper then falls, and the wheel is allowed to move through another sixth of a rotation, so as to carry on the tube into the dark chamber. A repetition of the making and unmaking of the magnet brings out the second tube, and shuts it up again ;

and another repetition does the like with the third tube. This apparatus, with a deep-sea lead attached to it, is suspended by an insulating cable that contains the wires whereby it is connected with the battery in the vessel. Being lowered down to any desired depth, the circuit is completed, the magnet made, and one of the tubes exposed for as long a time as may be wished; the circuit is then broken, the magnet unmade, and the tube shut up again. The second tube may be exposed for a longer time in the same place, or the apparatus may be lowered to a greater depth, at which the experiment may be repeated; and the third tube may then be dealt with in like manner.—The Committee having been satisfied with the performance of Mr. Siemens's apparatus, it had been arranged that trial should be made of it, and also of his Differential Thermometer, now provided with an improved Galvanometer; and he had undertaken to send out a qualified Assistant to take charge of these instruments during the Mediterranean Cruise. The declaration of war between France and Germany, however, unfortunately interfered with this arrangement; the Assistant (a German) being recalled to his own country, and no other competent person being available on a short notice. Under these circumstances it was thought better that the Differential Thermometer should not be sent out; but it was hoped that such a trial might be given to the Photometric Apparatus as should at any rate determine whether satisfactory results might be anticipated from its use, or whether any modifications in its construction might be needed. The apparatus was sent out to Gibraltar under charge of Dr. Carpenter, and was got into working order by his Son and himself in Gibraltar Harbour. It proved, however, that the action of sea-water on the bearings,—increased as this was by the galvanic current arising out of the contact of iron and brass in them,—so embarrassed its Mechanical arrangements, that no fair trial could be made of its Photometric efficiency. But the experiment served the important purpose of showing the weak points of the apparatus; and neither Mr. Siemens nor Dr. Carpenter entertains any doubt that it may be so reconstructed as to answer the purpose for which it was devised.

3. The work of this year's Expedition was divided, according to the plan originally marked out, into two Cruises: the *first* to examine the Deep-sea bottom between Falmouth and Gibraltar; the *second* to make the like examination of the western basin of the Mediterranean between Gibraltar and Malta, and to determine its Physical and Biological relations to the Atlantic, with special reference to the Gibraltar Current.—The First Cruise was under the scientific direction of Mr. Gwyn Jeffreys, who was accompanied by a young Swedish Naturalist, Mr. Josua Lindahl, of the University of Lund, as Zoological Assistant; whilst Mr. W. L. Carpenter, as before, took charge of the Chemical department,—his special work, on this occasion, being the determination, by Volumetric analysis, of the proportion of Chlorine in samples of Atlantic water taken from the surface, the bottom, and from intermediate depths, so as to serve as a basis of com-

parison with similar determinations of Mediterranean water.—In the Second Cruise it had been arranged that Dr. Carpenter and Prof. Wyville Thomson should co-operate as before; but the latter being unfortunately prevented by serious illness from taking part in it, the whole charge of this Cruise rested with Dr. Carpenter. He was fortunately able to retain the assistance of Mr. Lindahl; and the Chemical work was continued (as in the Third Cruise last year) by Mr. P. H. Carpenter. Mr. Laughrin throughout acted as dredger and sifter.

NARRATIVE.

FIRST CRUISE.

4. After leaving Falmouth on the 4th of July, a thick fog and contrary wind delayed our voyage to such an extent that Captain Calver considered it advisable to put into Mount's Bay, as we could make but little way, and were uselessly expending coal. We remained there at anchor all the next day. The wind having moderated, we started at daybreak on the 6th, and steamed westward. During the day we used a towing-net (constructed on a plan of Lieut. Palmer), while the vessel was going at a speed of between five and six knots an hour, and caught myriads of a small oceanic Crustacean, *Cetochilus Helgolandicus*.

5. In the evening of the next day (7th of July) we reached that part of the slope extending from the entrance of the British Channel to the Atlantic deeps, which appeared from the chart and our sounding to be promising ground; and here our first dredging was made in 567 fathoms (Station 1). There being little or no wind, the contents of the dredge were very small, but proved extremely interesting. Among the Mollusca were *Terebratula septata*, *Limopsis borealis*, *Hela tenella*, *Pecchiolia* (or *Verticordia*) *abyssicola*, and a fine species of *Turbo*, which we were subsequently enabled to identify with *Trochus filusus* of Philippi, of which his *T. glabratus* is a variety. The last-named species and its variety are only known at present as Tertiary fossils of Calabria and Messina. The three species first named likewise occur in the Pliocene beds of Southern Italy; and these, as well as the *Pecchiolia* or *Verticordia*, live in the Norwegian seas. The other species of Mollusca now dredged are also northern, with the exception of *Ringicula ventricosa* (one of our Crag fossils), which was obtained in last year's expedition, not far from our present position, in 557 fathoms. The Rev. Mr. Norman notices among the Crustacea new species of *Ampelisca* and of six other genera. Of Echinoderms the pretty *Echinus elegans* was the most conspicuous. We lay-to at nightfall, so as to keep near the same ground.

6. On the 8th the weather was very fine; but there was not sufficient wind to give the necessary driftway for dredging. Our first attempt in 305 fathoms (Station 2) was almost a failure. Later in the day dredgings in 690 fathoms (Station 3) and about 500 fathoms produced some important results, viz. MOLLUSCA: *Rhynchonella Sicula*, Seguenza MS. (a Sicilian

fossil), *Pleuronectia* sp. n., *Actæon* sp. n., besides *Limopsis borealis*, *L. aurita*, *Dentalium abyssorum*, *Puncturella noachina*, *Hela tenella*, *Rissoa Jeffreysi*, *Natica Montacuti*, *Admete viridula*, *Pleurotoma carinata*, and other northern species. CRUSTACEA: Mr. Norman reports as to the 690 fathoms (No. 3), "A most important dredging; the results among the Crustacea being more valuable than all the rest put together—at any rate of the First cruise. It contains almost all the choicest of the new species in last year's expedition, and four stalk-eyed Crustaceans of great interest, three of which are new, and the fourth (*Geryon tridens*) is a fine Norwegian species." And he adds that "with these are associated two forms of a more southern character, *Inachus Dorsettensis* and *Ebalia Cranchii*, which I should not have expected at so great a depth." ECHINODERMATA: *Cidaris papillata* (from which, according to Professor Wyville Thomson, *C. hystrix* is not specifically distinct), *Echinus elegans*, *Astropecten arcticus*, *A. Andromeda*, *A. Pareli*, and *A. irregularis*. ANNELIDA: Dr. M'Intosh notices, as a species supposed to be specially northern, *Thelepus circinnatus* of Fabricius from 690 and 500 fathoms. HYDROZOA: a new and beautiful tree-like form of a deep orange-colour. SPONGIA: *Holtenia Carpenteri* in considerable numbers and of all ages. Professor Wyville Thomson is fully convinced that the *H. Grayi* of Kent is only a variety of this species. The dredges did not fill; and most of the above results were obtained by means of the "hempen tangles," which were in 1869 for the first time attached to the dredge, and used with such wonderful success.

7. July 9th. Dredged all day; but the wind was too light, and the drift therefore insufficient for effective work. We began dredging in 717 fathoms (Station 4), and afterwards shifted the ground, getting 358 fathoms (Station 6), when we left off. This was about 185 miles from Cape Clear and Ushant, and 165 miles from the Scilly Isles. The Fauna was generally of a northern character, and included among the Mollusca *Terebratella Spitzbergensis* (Arctic and Japanese), *Pecten vitreus*, *P. aratus*, *Leda pernula*, *Axinus eumyarius*, *Rissoa turgida*, *Trochus suturalis* (a Sicilian fossil), *Odostomia nitens* (Mediterranean), *Taranis Mörchi*, *Defrancia* sp. n., and *Pleurotoma hispidula* or *decussata* = *P. concinnata*, S. Wood (Sicilian fossils, the last being Coralline Crag also), *Ringicula ventricosa*, *Actæon* sp. n., and *Bulla propinqua*. Some species were common to the North Atlantic and the Mediterranean. Among the Echinoderms was a fine specimen of *Brisinga endecacnemos*; and the Corals were represented by *Amphihelia oculata* and *Desmophyllum crista-galli*. Among the Annelids were *Pista cristata* of O. F. Müller, and *Trophonia glauca* of Malmgren, both Arctic species.

8. We lay-to on Sunday the 10th; and the next day we resumed our soundings and dredgings on the Channel slope at depths ranging from 257 to 690 fathoms (Stations 8, 9), occasionally changing the ground. The Fauna was everywhere northern, with a few exceptions. As to the Mollusca may be mentioned *Terebratula septata* (intermediate in shape between the typical kind and *Waldheimia Floridana*), *Leda* (*Yoldia*) *obtusa*,

Pecchiolia granulata, *Trochus suturalis*, *T. reticulatus*, *Rissoa subsculpta* (the last four Sicilian and Calabrian fossils), *Scalaria* sp. n., *Solarium fallaciosum*, *Fusus Berniciensis*, *F. fenestratus*, *Pleurotoma hispidula*, and *Bulla propinqua*. The Crustacea included *Cyclaspis longicaudata* (Norwegian) and *Polycheles typhlops* (Mediterranean), besides some new and peculiar species. A stony Coral of an undescribed genus and species also occurred, together with *Caryophyllia cyathus*, var. *clavus*. In the evening we steamed southwards, with a leading wind, for the deepest water in this part of the North Atlantic. We were afraid to continue the dredgings on the Channel slope towards the French coast, because the submarine telegraph-cable between Brest and North America might possibly be injured, and no information had been given, or could be obtained, as to the line of its direction.

9. July 12. On reaching the trough in the Bay of Biscay (or rather in that part of the Atlantic which lies outside the Bay), the sea became too high and the wind too strong for either sounding or dredging. This was from 250 to 300 miles south of the Scilly Isles, and about 200 miles north of Vigo. Our object was to get a cast in the greatest depth; and we lay-to all the day, waiting patiently for the chance of more favourable weather. But the wind did not take off at sunset, and the prospect did not improve; so it was determined not to lose any more time. At 10.30 p.m. steam was got up, and we went on towards Vigo. Rain fell at night; and the sea was brilliantly illuminated by the phosphorescent *Noctiluca* and other animals. Some of these, especially the smaller oceanic Hydrozoa, gave a much brighter and steadier light than the rest; so that they might fancifully be compared to planets among stars. The next day (13th) was fine overhead; but there was too much swell to have carried out our intention of dredging in the deepest water.

10. Thursday, July 14, passed Cape Finisterre, and dredged in 81 fathoms (Station 10), about nine miles from the coast of Spain. Fauna mostly southern; although Mr. Norman notices among the Crustacea a new species of *Mysis*, and the following British and Norwegian kinds, *Galathea Andrewsii* and *Orangon nanus*; and Dr. McIntosh gives *Terebellides Stræmii* and *Praxilla gracilis*, both Norwegian Annelids. We then steamed out, and dredged in 332 fathoms (Station 11). The bottom was rocky or stony; and the dredge fouled. On the tangles were two specimens (one adult and the other young) of that singular Echinoderm, or soft sea-urchin, belonging to the *Diadema* family, which was procured last year in nearly 60 degrees of North Latitude. It will be soon described by Professor Wyville Thomson under the name of *Calveria hystrix*. With this Echinoderm were the arms of *Brisinga endecacnemos*, and a specimen of a northern Mollusk, *Rissoa Jeffreysi*. Another dredge being put down on the same ground, was unfortunately lost, with some rope.

11. The following day (15th) we sounded in 128 and 232 fathoms (Stations 11, 12) about forty miles from Vigo, but used tangles only in consequence of the rocky nature of the bottom. The only noticeable Mollusk

was *Rissoa Jeffreysi*; and we also got an undescribed Polyzoon (*Idmonea Hispanica*, Busk), which was afterwards found in the Mediterranean.—16th. Dredged twice in Vigo Bay, at a depth of about 20 fathoms. This may be almost called “classical” ground; for it was the scene of Mr. M’Andrew’s dredging-operations in the spring and autumn of 1849. We obtained a few species of Mollusca new to this locality; and two of these (*Tellina compressa* and *Nassa semistriata*) are interesting, as having been described and figured by Brocchi from the Subapennine Tertiaries. The latter species is one of our Crag fossils, under Mr. J. Sowerby’s name of *Buccinum labiosum*. Mr. Busk mentions *Lepralia unicornis*, a Polyzoon previously known as from the Coralline Crag and Italian Pliocene, as well as Mediterranean. Vigo was our first anchorage after leaving England; and on Sunday we attended Divine Service, and dined with the late Capt. Burgoyne, on board his ill-fated but noble vessel the ‘Captain,’ which also had just arrived, after encountering some rough weather on her way out.

12. We left Vigo Bay at daybreak on Monday the 18th. It blew strong from the north-east; and after going about forty miles westward, and trying in vain to sound, we drifted along till the evening, and then steamed slowly in the direction of Lisbon, which was distant about 200 miles. The following day (19th) we sounded and dredged at depths of 100 and 220 fathoms (Station 13) from thirty to forty miles west of Cape Mondego, on the coast of Portugal. The Fauna at the lesser depth was southern and local, and at the greater depth comprised the following interesting species of Mollusca:—*Terebratula cranium*, *Limopsis borealis*, *L. aurita*, *Dentalium abyssorum*, *Trochus amabilis*, *T. suturalis*, *Trophon costifer* (Coralline Crag), *Fusus antiquus*, monstr. *contrarium*, *F. fenestratus*, and *Pleurotoma carinata*. Among the Foraminifera were specimens of the beautiful *Orbitolites tenuissimus* (sp. n., Carp.) found last year on the north-west of Ireland (Report, par. 36), and some other peculiar forms.

13. Wednesday, July 20th. Dredged all day with considerable success at depths of from 380 to 994 fathoms (Stations 14–16), the wind and sea having now gone down; and we took with the scoop-net a few living specimens of *Clio cuspidata*. The dredgings in 380 and 469 fathoms yielded among the Mollusca *Leda lucida* (Norwegian, and a Sicilian fossil; probably included in Philippi’s description of *Nucula pellucida*), *Axinus eumyariis* (also Norwegian), *Næra obesa* (Spitzbergen to the west of Ireland), *Odostomia* sp. n., *O. minuta* (Mediterranean), and *Cerithium* sp. n.; and among the Echinoderms were *Brisinga endecacnemos* and *Asteronyx Loveni*. But the results of the Dredging in 994 fathoms were so extraordinary as to excite our utmost astonishment. It being late in the evening, the contents of the dredge could not be sifted and examined until daylight the next morning. We then saw a marvellous assemblage of Shells, mostly dead, and consisting of Pteropods, but comprising certain species which we had always regarded as exclusively Northern, and others which Mr. Jeffreys recognized as Sicilian Tertiary fossils, while nearly forty per cent. of the entire number of species were undescribed, and some of them repre-

sented new genera. The following is an analysis of the Mollusca (perfect and fragmentary) taken in this one haul :—

Orders.	Total number of species.	Recent.	Fossil.	New or undescribed.
Brachiopoda	1	1		
Conchifera	50	32	1	17
Solenocoelia	7	3	..	4
Gastropoda	113	42	23	48
Heteropoda	1	1		
Pteropoda	14	12	..	2
	186	91	24	71

The Northern species above referred to are 34 in number, and include *Mytilus (Dacrydium) vitreus*, *Nucula pumila*, *Leda lucida*, *L. frigida*, *Pecchiolia abyssicola*, *Neæra jugosa* or *lamellosa*, *N. obesa*, *Tectura fulva*, *Fissurisepta papillosa*, *Cyclostrema* sp. n., *Torellia vestita*, *Pleurotoma turricula*, *Admete viridula*, *Cylichna alba*, *Cylichna ovata*, Jeffr. MS. = *Bulla conulus*, S. Wood, not Deshayes (Coralline Crag), and *Scaphander librarius*. *Leda lucida*, *Neæra jugosa*, *Tectura fulva*, *Fissurisepta papillosa*, *Torellia vestita*, and the undescribed species of *Cylichna*, as well as several other known species in this dredging, are also fossil in Sicily. Nearly all these Shells, as well as a few small Echinoderms, Corals, and other organisms, had evidently been transported by some current to the spot where they were found ; and they must have formed a thick deposit, similar to those of which many Tertiary fossiliferous strata are composed. It seemed probable also that the deposit was partly caused by tidal action, because a fragment of *Melampus myosotis* (a littoral Pulmonibranch) was mixed with deep-water and oceanic Pectinibranchs and Lamellibranchs. None of the shells were Miocene, or of an older period.

14. This remarkable collection, of which not much more than one half is known to Conchologists, notwithstanding their assiduous labours, teaches us how much remains to be done before we can assume that the record of Marine Zoology is complete. Let us compare the vast expanse of the seabed in the North Atlantic with that small fringe of the coast on both sides of it which has yet been partially explored, and consider with reference to the dredging last mentioned what are the prospects of our ever becoming acquainted with all the inhabitants of the deep throughout the globe ! We believe, however, that a thorough examination of the newer Tertiaries would materially assist us in the inquiry ; and such examination is feasible and comparatively easy. Much good work has been done in this line ; but although the researches of Brocchi, Bivona, Cantraine, Philippi, Calcara, Costa, Aradas, Brugnone, Seguenza, and other able Palæontologists in the South of Italy have extended over more than half a century, and are still energetically prosecuted, many species of Molluscons shells are con-

tinually being discovered there, and have never been published.—Besides the Mollusca in this dredging from 994 fathoms, Professor Duncan informs us that there are two new genera of Corals, and *Flabellum distinctum*, which last he regards as identical with one from North Japan. It coincides with the discovery on the Lusitanian coasts of two Japanese species of a curious genus of Mollusca (*Pecchiolia* or *Verticordia*), both of which are fossil in Sicily, and one of them in the Coralline Crag of Suffolk. Professor Wyville Thomson notices undescribed Sponges from the same dredging. The weather was very hot, and the sea quite smooth, at 10 P.M.

15. Thursday, July 21st. On deck at 5 A.M. Dredged all day in from 600 to 1095 fathoms (Stations 17, 17 *a*) with extraordinary success. Together with many of the new and peculiar species of Mollusca obtained at Station 16 (994 fathoms), some of which were here alive, occurred—*Nucula delphinodonta*, *Leda* (*Yoldia*) sp. n., *L. abyssicola*, *Axinus eumyarius*, *Siphonodentalium vitreum* (the first being North-American and Norwegian, and the last three Arctic and Norwegian), *S. coarctatum* (a well-known Subapennine fossil), *Dischides* sp. n., *Chiton albus* (northern), *Molleria costulata* (Arctic), *Trochus reticulatus* (a Calabrian fossil), *Omphalius monocingulatus*, Seg. MS. (a Sicilian fossil), *Hela* sp. n., *Eulima* sp. n., *Scalaria frondosa* (a Leghorn and Crag fossil), and *Trachysma delicatum* (a Sicilian fossil). Of Crustacea, there were *Apseudes spinosa* (Norwegian and British), *A. grossimanus* (sp. n.), and *Paranthura elongata* (sp. n.). Of Polyzoa, *Cellepora abyssicola*, sp. n. (Busk, MS.). Of Corals, *Cænocyathus* sp. n., and an undescribed species of an unknown genus allied to *Bathycyathus*. *Holtenia Carpenteri* and other rare Sponges, with *Brisinga endecacnemos* and various Echinoderms equally interesting, formed part of our treasures; but the greatest prize of all was a noble *Pentacrinus*, about a foot long, of which several specimens came up attached to the tangles. This discovery of a true *Pentacrinus* in the European seas crowned the day's work. Mr. Jeffreys has named it *P. Wyville-Thomsoni*; Dr. Carpenter will describe it, and give its zoological and geological relations, as he is especially acquainted with this group of the Echinoderms by having worked out the structure of *Antedon* in the Philosophical Transactions for 1866. We may remark that our *Pentacrinus* was dragged up from soft mud or ooze, and that its base was entirely free. Portions of the arms occurred in several other dredgings on the Lusitanian coasts; and joints of apparently the same species have been found by Professor Seguenza in the Zanclean formation or older Pliocene near Messina.

16. July 22nd. We tried to dredge among the Berling Isles, but could do nothing. The ground was rocky, and the charts were incorrect. The sounding-lead was deeply indented, and a water-bottle torn away and lost. A dredge was afterwards put down twice in a trough or gulley between 900 and 1000 fathoms deep; each time it came up empty. We then steamed for Lisbon, where we arrived the next day.

17. On the 25th (Monday) we got to Cape Espichel. The wind had in-

creased so much that, after fruitlessly endeavouring to dredge, we anchored in the evening for shelter in Setubal Bay. We there dredged, with no special result. Professor Bocage had kindly given us at Lisbon a letter of introduction to the coast-guard Officer at Setubal, who was said to know the only place where the deep-sea Shark and the *Hyalonema* are taken by the fishermen; but the state of the weather unfortunately prevented our availing ourselves of it.

18. July 26th. Although the wind was rather high and the sea rough, we contrived (owing to the admirable management of Captain Calver) to dredge off Cape Espichel in 740 fathoms (Station 20); and later in the day, having stood out further to sea, in 718 fathoms (Station 22.). Deeper water is sometimes found to be near land than at a distance from it. The Mollusca were mostly of the same kind as those from No. 16 (994 fathoms), but included *Leda pusio*, *Limopsis pygmaea* (Sicilian fossils), and *Pecchiolia acuticostata*. The last-named species is extremely interesting in a geological as well as geographical point of view. It is a fossil of our Coralline Crag and the Sicilian pliocene beds; and it now lives in the Japanese archipelago. Some Japanese Brachiopods and Crustacea also inhabit the Mediterranean. It may be difficult to account for this migration to or from Northern Asia, except through the Arctic ocean; but we would again venture to call attention to the suggestion made by Mr. Jeffreys as to the communication which probably existed, at a period subsequent to the Middle Tertiary or Miocene epoch, between the North Atlantic and the Mediterranean, in the direction of the Languedoc canal or Canal du Midi, from the Bay of Biscay to the Gulf of Lyons. The Straits of Gibraltar do not appear to afford the means of such migration or transport of any northern Fauna to the Mediterranean, because the current which flows into it from the Atlantic is superficial only, and does not reach the bottom, which the *Pecchiolia* inhabits. This Mollusk has no power of swimming, like many univalves; nor does its fry rise to the surface and become for a short time oceanic, as in certain species of *Trochus* and *Dolium*. Any current which flows out of the Mediterranean at its bottom into the Atlantic would transport the fry of the *Pecchiolia* southwards, or at any rate could not withstand the great northern current which brings Arctic mollusca to the Lusitanian seas. Now the greatest height above the sea in the line of the Canal du Midi from Bordeaux to Narbonne is stated to be 189 mètres, or about 615 feet. M. Reboul, in his "Mémoire sur les Terrains de comblement tertiaire" (Mém. Soc. Géol. de France, 1834) mentions, as to the district in question, marine tertiary shells which had been deposited by a sea higher by about 200 mètres than the present sea; and he says that M. Deshayes considered these shells to be among the most recent of the Tertiary period. No lists of the shells, or any definite particulars of the deposits, however, have been published by French geologists; and it is to be hoped either that this deficiency will be supplied by them, or that Mr. Prestwich may be able to investigate the matter with his usual

ability, and give us some satisfactory and reliable information. In the dredging (Station 22) at 718 fathoms, Mr. Norman reports new species of Crustacea belonging to four genera.

19. July 27th. Dredged a few miles north of Cape St. Vincent, in 292 and 374 fathoms (Stations 24, 25). Ground rocky; and in the evening a dredge, with 400 fathoms of 3-inch rope, was unavoidably lost. The last haul yielded two Siliceous or Vitreous Sponges of an enormous size, one of them measuring nearly 3 feet in diameter at the top, of the kind called "Neptune's Cup" (*Askonema Setubalense*, Kent), besides that lovely sponge, *Aphrocallistes Bocagei*. The Mollusca were mainly northern, and included fresh-looking fragments of the gigantic *Lima excavata*, besides *Limopsis minuta* (distinct from *L. borealis*), *Pecchiolia acuticostata*, *P. granulata*, *Trochus suturalis*, and *Pleurotoma hispidula* or *decussata*, all of which are Sicilian fossils. Two undescribed species of Crustacea, which Mr. Norman proposes to name *Amathia Jeffreysi* and *Ethusa mirabilis*, were here obtained.—At night we steamed slowly south, and doubled Cape St. Vincent. The electric telegraph-cable between Falmouth and Gibraltar sadly hampered our movements in this part of the cruise, by occupying the ground which we were most desirous of exploring. It was not considered safe to dredge within eight or ten miles on either side of it. This was a serious drawback; since it obliged us to dredge either in water which is too deep for systematic or continued exploration, or in comparatively shallow water near the coast, where the ground is rocky and the dredge liable to be lost.

20. July 28th. Dredged several times off Cape Sagres in from 45 to 58 fathoms. Fauna southern. *Venus multilamella*, *Tellina compressa*, and *Halia Priamus* occurred living; and Dr. M'Intosh says that two Annelids (*Glycera capitata* and *Praxilla prætermissa*) had not been hitherto observed south of the track in last year's expedition. The sea being rough and wind high, we anchored off Lagos.

21. July 29th. Steamed south, and dredged in 364 and 322 fathoms (Stations 26, 27). The water was of an indigo-blue colour. Weather fine, but rather windy. For the first dredging 700 fathoms of rope were payed out, and two weights of 100 lbs. were attached at 350 fathoms from the dredge. The Mollusca comprised some of the new and remarkable species procured in Station 16 (994 fathoms) and other dredgings, as well as *Terebratula vitrea*, *T. cranium*, *Pholadomya* sp. n., *Trochus amabilis*, *Pyramidella plicosa* (Belgian and Coralline Crag), *Tylodina Dubeni* (Norwegian), *Cancellaria mitræformis*, *C. subangulosa* (both Coralline Crag), *Pleurotoma galerita*, and *Actæon pusillus*. The Crustacea of most interest were *Pagurus platycheles* (sp. n.), and an undescribed species of *Munna*. The Corals were *Flabellum distinctum* (the same as in Station 16) and *Amphihelia oculata*. The Hydrozoa in 364 fathoms included a new and beautiful species of *Plumularia*. But the most remarkable novelty here obtained was a large collection of thin sandy disks, from 0.3 to

0·4 inch in diameter, with a slight central prominence; for these proved on subsequent examination to contain an entirely new type of Actinozoon, extraordinarily flattened in form, and entirely destitute of tentacles. Dr. Carpenter, by whom this curious organism will be described, has assigned to it the name of *Ammodiscus Lindahli*.

22. July 30th. Sounded and dredged at two stations on our way to Cadiz in 304 and 280 fathoms (Stations 28, 28a). At the first of these stations *Flabellum distinctum* and a new species of *Caryophyllia* occurred. At the latter station we got the same undescribed species of *Pholadomya*, being the second species known in a recent or living state; the other is extremely rare, and West Indian. With the *Pholadomya* were *Trochus crispulus* and *Odostomia plicatula* (Sicilian fossils), and *Actæon exilis*, besides undescribed species of *Poromya*, *Mitra*, and *Marginella*. Anchored off Cadiz, near H.M.S. 'Cruizer;' where we had at first some difficulty in being allowed to land, in consequence of our not being provided with a bill of health, and there being no surgeon on board.

23. Left Cadiz on Tuesday, 2nd August, and steamed west, so as to get on the seaward side of the provoking cable. Dredged in 227 and 386 fathoms (Stations 29, 30). There was an admixture of northern and southern species, including a fragment of *Fusus antiquus*, var. *carinata*; and the *Ammodiscus* here also presented itself, with a test composed of coarser sand-grains than before, and frequently including Foraminifera.

24. Aug. 3rd. Dredged at three more Stations (31, 32, 33), in 477, 651, and 554 fathoms, across the entrance to the Straits of Gibraltar, and towards the Morocco coast; and we shifted our ground at night. The Fauna was northern, but scanty; the bottom being stiff clay, and nearly unproductive. Undescribed species of *Cioniscus* and *Bulla* were among the Mollusca; a remarkable Sponge, eighteen inches long, which Prof. Wyville Thomson considers the type of a new genus allied to *Esperia* (*Chondrocladia virgata*, Wyv. Th. MS.), another new and exquisitely graceful Sponge of the *Holtenia* group, and provisionally named by him *Pheronema? velatum*, and *Aphrocallistes Bocagei*; two new species of a compound stony Coral (*Cænocyathus*); and a few Crustaceans and Annelids were taken. Part of a large Ribbonfish (*Regalecus gladius*) was caught floating on the surface of the sea, the remainder having been apparently bitten off and devoured by a Shark or Swordfish.

25. Aug. 4th. Dredged again off the Straits and on the coast of Africa in from 414 to 72 fathoms (Station 34). In the greater depths was the same stiff and nearly azoic clay; and in the lesser depths were broken and dead shells of southern species. The Fauna in the former was chiefly northern, and comprised *Rissoa turgida* (Norwegian) and *Holtenia Carpenteri*. Capt. Spratt suggests that the clayey bottom may have been formed by continual deposits from the great and muddy rivers Guadalquivir and Guadiana. Such deposits would to a considerable extent inter-

fere with the existence and growth of marine animals. An easterly wind having sprung up, we hove-to off Cape Spartel, between thirty and forty miles from Gibraltar.

26. Aug. 5th. Steamed into Tangier Bay, after ineffectually trying to dredge in 190 fathoms (Station 37) off Cape Spartel. The bottom in this part of the Straits is everywhere rocky; and there is reason for believing that it must be swept by the undercurrent in the middle, and by the tide at the sides. Sargasso or Gulf-weed was found floating off the Cape. In Tangier Bay we dredged twice at a depth of about 35 fathoms. Fauna principally British, with a few more southern forms: the last include, among the Mollusca, *Pythina?* *Macandreae*, *Cyclostrema sphaeroïdeum* (Coralline Crag), *Rissoa sculpta* and *R. substriata* (Calabrian and Sicilian fossils), *Adeorbis supranitidus* (Coralline Crag); and among the Polyzoa, *Cupularia Canariensis*, which inhabits Madeira and the Canaries, and occurs in the Coralline Crag as well as other tertiaries of the same age. *Sphenotrochus intermedius* (a Coralline Crag Coral) was the only other acquisition worth notice.

27. On the following day (6th August) we arrived at Gibraltar. Mr. Jeffreys was there succeeded by Dr. Carpenter; and the former went on to Sicily *via* Malta, for the purpose of examining the newer tertiary formation in the south of Italy, and the collections of fossil shells at Catania, Messina, Palermo, and Naples, in connexion with the results of his cruise.

28. The quantity and variety of Zoological materials is so great that we have distributed it as follows:—*Crustacea*, Rev. A. M. Norman and Mr. George S. Brady; *Polyzoa*, Mr. Busk; *Annelida*, Dr. M'Intosh; *Corals or Stony Anthozoa*, Professor Duncan; *Horny or Flexible Anthozoa*, Mr. Kent; *Hydrozoa*, Dr. Allman; *Echinodermata and Sponges*, Professor Wyville Thomson. The *Mollusca* will be worked out by Mr. Jeffreys; and the *Pentacrinus*, *Ammodiscus*, and *Foraminifera* by Dr. Carpenter, who also undertakes to discuss the Physical results.

29. Throughout the whole of this Cruise the Temperature of the *Seabottom* was taken by the protected Miller-Casella Thermometers in nearly every Sounding, with the results tabulated in p. 220. As, for the reason already mentioned (§ 9), no extreme depths were sounded, and as the general rate of the diminution of temperature on the margin of the North-Atlantic basin seemed to have been established by the Serial Soundings taken in the Expedition of the preceding year, it was not thought necessary to repeat these; more especially as the variety of depths at which the *Bottom*-temperatures were ascertained gave adequate data for comparison with the results then correlated. It will be shown hereafter (§ 79) that this comparison leads to some very interesting conclusions, fully confirming the view advanced in the last Report (§ 119) as to the slow northward movement of an upper stratum of warm water 700 or 800 fathoms in depth, and of the southward movement of the whole deeper stratum, bringing water of an almost icy coldness from the Arctic basin into the Temperate and even the Intertropical Zone.

30. During the whole of this Expedition the Temperature of the *surface* of the Sea was ascertained and recorded every two hours, both by day and by night; as were also the readings of the Dry and Wet-bulb Thermometers, which were placed in a small penthouse on deck, in which they were freely exposed to the surrounding Air, but secluded from direct or reflected Solar heat.—The Temperature of the *surface-water*, from the time of our leaving the British Channel in Lat. 48° N. to our turning the corner of Cape St. Vincent in Lat. $36^{\circ} 50'$ N., increased at a rate which bore a pretty regular proportion to the Southing. Thus, at the “chops of the Channel,” it averaged 62° for five days; whilst by the time we approached Cape St. Vincent it had gradually risen to above 69° . After passing that point, however, we found both the *surface*- and the *bottom*-temperatures to present certain variations, which, though not considerable in themselves, proved to be of great interest when taken in connection with the peculiar condition of the *embouchure* of the Strait of Gibraltar. These points, however, will be more fitly discussed hereafter (§ 73 *et seq.*); and we shall now only notice a sudden *rise* in Surface-temperature of about 3° which showed itself as we turned the corner of Cape St. Vincent and entered the *north side* of the embouchure, and a sudden *fall* of nearly 6° (to $66^{\circ} \cdot 4$) which was encountered when we entered the mid-stream of the narrower part of the Strait as we proceeded towards Gibraltar.

31. In the course of the First portion of the Cruise between Falmouth and Lisbon (beyond which point Mr. W. L. Carpenter was unable to proceed), thirty-six quantitative determinations were made, by Volumetric analysis, of the amount of Chlorine in as many samples of Atlantic water, taken (1) from the surface, (2) from the bottom at various depths, and (3) from various intermediate depths. The greater part of these, as will be shown hereafter (§ 84), exhibited a very close conformity to a uniform standard of density, as indicated by a Specific Gravity of 1.0268, and a Chlorine proportion of 19.84 per 1000*; the chief departures being observable in the *lower* density of the *deepest* waters, and in the occasional *excess* of density in the surface-waters. The former is doubtless attributable to the fact that the *deepest* water is essentially *Polar*, and therefore derives its more dilute character from that source. The latter we are inclined to attribute to the influence of slight concentration by evaporation.

SECOND CRUISE.

32. Leaving Gibraltar early in the morning of Monday, Aug. 15, we steamed out into the middle of the Strait, for the purpose of commencing our experiments on the Gibraltar Current. The point selected by Capt. Calver (Chart of Strait of Gibraltar, Station 39) lay midway between Point Carnero, which forms the south-eastern boundary of Gibraltar Bay,

* The proportion here adopted,—the number of Grammes of Chlorine to 1000 Cubic Centimètres of Water,—is that employed by Prof. Forchhammer in his *elaborate Memoir on the Composition of Sea Water* (Philos. Transact. 1865).

and Jebel Musa or Apes Hill, which lies opposite to it, at a distance of only 8 geographical ($9\frac{1}{4}$ statute) miles, on the African coast, the Strait being here nearly at its narrowest: and it was also that at which the greatest depth (510 fathoms) was indicated by the Soundings marked on the Chart. With this depth our own Sounding, which gave a bottom at 517 fathoms, agreed very closely; and having thus at once found the position most advantageous for our work, that position was precisely determined by angles taken by sextant from the Ship between conspicuous objects on the shore. The *bottom*-temperature obtained in the first sounding was between 5° and 6° *higher* than that which had been met with at corresponding depths on the bed of the Atlantic about 100 miles to the westward; whilst the *surface*-temperature was *lower* by from $5^{\circ}\cdot3$ to 6° , as will be seen by the following comparative statement:—

	Station.	Depth.	Surface- temperature.	Bottom- temperature.
Strait of Gibraltar..	39	517	$66^{\circ}\cdot0$	$55^{\circ}\cdot5$
Atlantic	31	477	$71^{\circ}\cdot3$	$50^{\circ}\cdot5$
Atlantic	32	651	$71^{\circ}\cdot5$	$50^{\circ}\cdot0$
Atlantic	33	554	$72^{\circ}\cdot0$	$49^{\circ}\cdot7$
Atlantic	34	414	$71^{\circ}\cdot7$	$50^{\circ}\cdot0$

33. This striking difference led us to take a set of *serial* soundings at intervals of 50 fathoms; and these gave a result which, though it appeared anomalous at the time, was afterwards fully explained, and proved to be of unexpected import. The Temperature fell, at 50 fathoms from the surface, to 56° ; at 100 fathoms it was $55^{\circ}\cdot7$; at 150 it was $55^{\circ}\cdot5$; and from that depth to the bottom, at 517 fathoms, there was *no further descent*. Now it will be shown hereafter (§ 88) that the thermal condition which here so much surprised us by its contrast with that of the Atlantic waters, is that universally met with in the Mediterranean; the Temperature of which, whatever may be its surface-elevation, falls to within 1° Fahr. above or below 56° at a depth of 50 fathoms, to a degree or two lower at 100 fathoms, and then remains uniform down to the greatest depth (1743 fathoms) at which we examined it. And it thus appears that whilst the *surface*-water in this part of the Strait is certainly derived from the Atlantic, the *deeper* water, partaking of the thermal condition which so remarkably characterizes that of the Mediterranean basin, may be fairly regarded as belonging to the latter.

34. This inference is in harmony with another fact ascertained on the same occasion, viz. the great excess in Salinity shown by water brought up from the depth of 250 fathoms over the water of the surface. Whilst the Specific Gravity of the latter was found to be 1·0271, that of the former was 1·0293; and whilst the proportion of Chlorine in the latter was 20·324 per 1000, it was 21·775 in the former. Now in these particulars the Surface-water agreed well with what had been found to be the condition

of the water of the Atlantic ; whilst the water at 250 fathoms agreed equally well with what proved to be the condition of the bottom water in the adjacent part of the Mediterranean (§ 43). We were not a little surprised, however, to find that the water here taken from the *bottom* (517 fathoms) was of much *less* density, as indicated both by Specific Gravity and by Chlorine percentage, than that of the *intermediate* stratum ; its Specific Gravity being 1·0281, and its proportion of Chlorine 21·465. This apparent anomaly (the existence of which was confirmed by observations made on our return voyage, § 61) pointed to the existence of an *out-current* in the *intermediate* stratum as the probable explanation of the overlaying of the lighter by the heavier water. The Specific Gravity of the *bottom*-stratum closely corresponded, as we subsequently found, with that of the bottom-water over the *deepest* part of the area of the Western basin of the Mediterranean (§ 93).

35. These data having been obtained by the examination of the several parts of the vertical column at one and the same point, and this point being in the centre of nearly the narrowest part of the Strait, and at the deepest part of the channel, we proceeded to test the *actual movement* of water on the surface and at different depths beneath it.

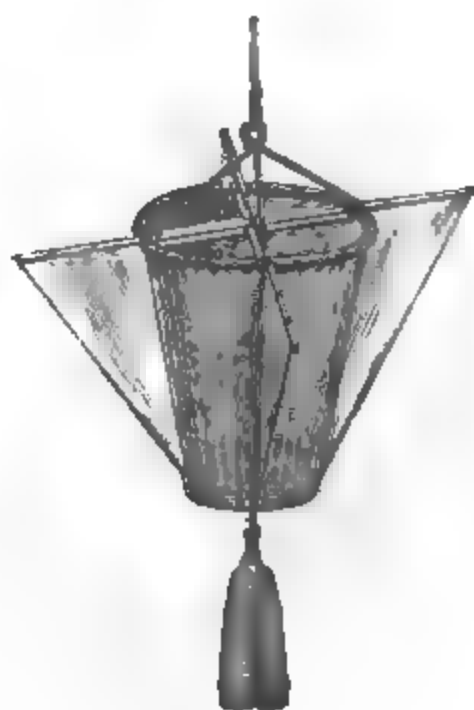
36. The rate of *surface*-movement was easily determined. The precise position of the Ship having been ascertained in the manner already stated, a small flat basket, presenting no such elevation above the water as would cause it to be influenced in any considerable degree by a moderate wind*, was sent adrift, so as to be freely carried along by the current ; it was allowed to float for a determinate time, throughout which it was followed by the ship ; and when it was taken up at the expiration of that time, the place of the ship was again ascertained as before. The space between the two points being then determined trigonometrically, the rate of the flow per hour, and its precise direction, could be readily calculated. Thus on the morning of August 15th the float was followed by the ship for fifteen minutes, during which it was found to have moved 4377 feet in the direction E. by S. $\frac{3}{4}$ S., or at the rate of 2·88 miles per hour (§ 40).

37. For the determination of the *movement of the water at different depths below the surface*, a "current-drag" (see figure, p. 165) had been constructed by Capt. Calver on a plan suggested by his previous experience, which had led him to the conclusion that a submerged basket lined with sailcloth, which of course fills itself with water, presents a better resisting surface than any vessel of wood or metal. Such a basket being made the basis (so to speak) of the apparatus, its resisting surface was augmented by fixing two pairs of arms at right angles to one another across

* It is obvious that the movement of the Ship itself would be liable to be considerably affected by even a slight breeze, on account of the large surface of resistance presented by its transverse section (especially by its paddle-boxes) above the water. This would cause its drift to be *more* rapid than the current, if the direction of the wind should be *with* that of the current, and *less* rapid if the wind should be opposed to it.

its upper end, and by stretching a piece of sail-cloth between each arm and the side of the basket; which device caused a uniform resisting surface to be presented to the current, whatever the manner in which the sails might meet it. To the lower part of this "drag" a couple of sinkers, of 112 lbs. each, were attached; and the whole apparatus was supported by cords meeting in a ring above it, to which the suspending line was secured.

38. This "current-drag" having been transferred to a boat, was lowered down by a couple of men placed in her, to the desired depth; and the boat was then left entirely free to move, being lightened by the return of the men into the ship. The motion of the boat would be the composite result of (1) the action of wind (if any) upon the transverse section of the part of of the boat above the water; (2) the action of the surface-current upon the transverse section of the immersed part of the boat; (3) the action of the upper current upon the suspending line; and (4) the action of the current in which the "drag" is suspended upon the drag itself. Putting aside the *first* of these agencies, which will be of very little account if (as in the experiment now narrated) the boat be small and the breeze be light, it is obvious that the relative influence of the *second* and *third* to that of the *fourth* will depend upon the proportion between the surfaces presented by the boat, the line, and the "drag" respectively, and the strength of the current acting upon each. The surface given to the "drag" being larger than that of the boat and line taken together, the force acting on the "drag" will dominate, if it hang in an opposing current superior, equal, or even somewhat inferior in rate to that which acts on the boat and line; so that the boat would be carried along by the drag *against* the surface-stream, at a rate proportioned to the excess.—If, again, the rate of the undercurrent should be greatly inferior to that of the surface, its action upon the "drag" might still be sufficient to neutralize that of the surface-current upon the boat and line, and the boat would then remain *stationary* or nearly so.—A still further reduction in the rate of the opposing undercurrent would make its action upon the "drag" *less* powerful than that of the surface-current upon the boat and suspending line; and the boat would then move *with* the surface-current, but at a rate of which the great retardation would indicate an antagonistic force beneath.—Supposing, again, the water of the stratum in which the "drag" is suspended to be stationary, the action of the surface-current upon the boat and line would be opposed by the resistance offered by the deeper water



Current-drag.

to the movement of the drag; and the retardation of the movement of the boat would be less, though still considerable.—If, again, the stratum in which the “drag” is suspended should itself be moving in the direction of the surface-current, but at a reduced rate, there will still be a resistance to the movement of the “drag” at the more rapid rate of the surface-current; and this resistance will produce a proportional retardation in the motion of the boat.—Finally, if the stratum in which the “drag” is suspended, with the intermediate stratum through which the suspending line passes, move at the same rate with the surface-current, the motion of the boat with the whole suspended apparatus will have the same rate as that of the simple float.

39. Putting these respective cases conversely, it may be affirmed (1) that if the boat, having the “current-drag” suspended from it, should move *with* the surface-current and *at the same rate*, the stratum in which the “drag” hangs may be presumed to have a motion nearly corresponding with that of the surface-current; (2) that if the rate of movement of the boat *with* the surface-current should be *retarded*, a diminution of the rate of the stratum in which the “drag” hangs, to a degree *exceeding* the retardation of the movement of the boat, may be safely predicated; (3) that when this retardation is so considerable that the boat moves *very slowly* in the direction of the surface-current, it may be inferred that the stratum in which the “drag” is suspended is either stationary or has a slow movement in the opposite direction; (4) that if the boat should remain *stationary*, a force must be acting on the “drag” which is equal and in the *contrary direction* to that of the upper current upon the boat and suspending line; so that the existence of a counter-current is indicated, having a rate as much *less* than that of the surface-current, as the resisting surface presented by the “drag” is *greater* than that offered by the boat and upper part of the suspending line; (5) that if the boat should move in a direction *opposed* to that of the surface-current, a motion is indicated in the stratum in which the “drag” hangs which will correspond in *direction* with that of the boat, and which will *exceed* it in *rate*, the effect of the “drag” upon the boat being partly neutralized by the antagonistic drift of the surface-current.

40. Now our first set of experiments (Station 39) with the “current-drag” gave the following results:—

I. The surface-movement being first tested in the manner already described (§ 36), its rate was found to be 2·88 nautical miles per hour, and its direction E. by S. $\frac{3}{4}$ S. The wind was W. by N., with a force of 4.

II. The “drag” having been lowered down to a depth of 100 fathoms, the rate of movement of the boat from which it was suspended was reduced to 1·550 mile per hour, or *rather more than half* the surface-movement. Its direction was E. $\frac{1}{3}$ S. Taking into account the action of the wind and surface-current on the boat, it may be safely affirmed that at 100 fathoms the rate of the current was reduced to *less than one half*.

III. The "drag" having been lowered down to a depth of 250 fathoms, the boat remained nearly stationary, its *rate* of movement being reduced to 0.175 mile per hour, while its direction (S.E. $\frac{1}{4}$ E.) was slightly altered to the southward, though still easterly. From this we felt ourselves justified in inferring that the 250-fathoms' stratum had a movement in the *reverse*

Station 39.



Rate (per hour) and Direction of Movement of Surface-float, and of Current-drag at different Depths; with Force and Direction of Wind.

direction, acting on the current-drag with a force almost sufficient to neutralize the action of the upper stratum on the boat and suspending line. And this inference, which was strengthened by the indication already shown to be afforded by the extraordinary density of the water of this stratum (§ 34), was fully justified by the results of the experiments which we made on our return voyage (§ 62).

41. While these experiments were in progress, we had the pleasure of seeing the Channel Fleet, which was expected to meet the Mediterranean Fleet at Gibraltar, come in sight beyond Cape Tarifa; its approach having been indicated, long before even the tops of the masts of the vessels composing it showed themselves above the horizon, by the number of separate puffs of smoke which the experienced eye of our Commander enabled him to distinguish. As soon as all possibility of doubt was removed by the appearance of the masts, Capt. Calver communicated "Fleet in sight" by signal to the Admiral in Gibraltar Harbour, our position being such that *we* could be seen by him, though the Fleet could not. In due time, the massive hulls of the Ironclads rose above the horizon; and whilst we continued at our work, all passed us in sailing order at a distance of not more than a couple of miles,—the ill-fated 'Captain' being the chief object of interest. A few hours later, the 'Monarch,' which had been detained for repair, but whose passage had been made in a shorter time by the free use of her steam-power, came in sight; and passed on in solitary grandeur to join the Fleets now united in Gibraltar Bay.

42. The whole of our first day having been consumed without our being able to work the "current-drag" in the deepest stratum, we anchored for the night near Point Carnero, with a view to resuming our experiments on

the following morning. We then ran out to a spot almost precisely identical with that which had been our starting-point on the previous day ; and commenced, as before, by testing the rate and direction of the surface-movement. Its rate proved rather slower, being 2·40 miles per hour, instead of 2·88 ; and its direction was E. by N., instead of E. by S. $\frac{3}{4}$ S. Both differences seemed to be accounted for by the difference in the force and direction of the wind ; which, having been W. by N. with a force of 4 on the previous day, was now W. $\frac{1}{2}$ S. with a force of only 2. The "drag" was then lowered to a depth of 400 fathoms ; but our expectation that it would there encounter a westerly (or outward) current sufficiently strong to carry the boat in that direction in spite of the antagonistic movement of the easterly (or inward) surface-current, was not verified on this occasion ; for the boat slowly drifted in an E. $\frac{1}{4}$ N. direction, its rate being 0·650 mile per hour. Whether this result should be taken to indicate a stationary condition of the deep stratum, or a slight movement in either direction (§ 39), could scarcely be affirmed with positiveness ; but from the indication afforded by the Specific Gravity of the water taken up from this depth (§ 34), it seemed probable that the general movement of this stratum was at this time rather *westerly*, or in conformity with that which we attributed to the intermediate stratum, though at a slower rate.—It will be shown hereafter (§ 62) that a decisive proof of such a movement was obtained on a subsequent occasion.

43. Thinking it expedient to postpone the further prosecution of this inquiry until our return voyage,—when we should be able to repeat our experiments, not only at this narrow end of the Strait, but also at that shallowest portion to the westward where the Strait opens out into the Atlantic,—we put steam on before mid-day, and entered the basin of the Mediterranean, directing our course in the first instance to the spot (Lat. $36^{\circ} 0' N.$, Long. $4^{\circ} 40' W.$) at which the sample of bottom-water had been obtained by Admiral Smyth, which, when analyzed by Dr. Wollaston, was found to possess the extraordinary Specific Gravity of 1·1288, and to yield a percentage of 17·3 of Salt *. As we were within sight of both shores, and could distinguish several remarkable mountain-summits which were accurately laid down on our Charts, the bearings of these enabled the situation of the Ship to be determined with great precision ; and Capt. Calver undertook to place her within a mile of the point at which Admiral Smyth's observation had been taken. Having reached this (Station 40) we took our first Sounding in the Mediterranean ; and awaited the result with no little interest. The depth proved to be 586 fathoms, or 84 fathoms less than that given by Admiral Smyth's sounding ; but as the latter was not taken on the improved method now adopted, and as its correctness may have not improbably been affected by the strength of the easterly current which is here very perceptible, the discrepancy can scarcely be considered as of any real account as showing that the two points were otherwise than nearly

* Phil. Trans. for 1820, p. 29 ; and Admiral Smyth's 'Mediterranean,' pp. 128–130.

coincident *. The specimen of bottom-water brought up by our bottle was found to have a Specific Gravity of 1·0292, whilst that of the surface-water was 1·0270. The volumetric determination of the Chlorine gave 21·419 per 1000 for the bottom-water, as against 20·290 per 1000 for the surface-water. A decided excess of salt is thus indicated in the bottom-water, as compared on the one hand with the surface-water of the same spot, and on the other with the bottom-water of the Atlantic, which had been generally found to show a rather smaller proportion of Chlorine than the surface-water. But this excess is extremely small in comparison with that indicated by Dr. Wollaston's analysis. For, assuming his factor of ·134 as representing, when multiplied by the excess of Specific Gravity above that of distilled water, the total percentage of Salt, that percentage is only 3·91, instead of 17·3 as stated by Dr. Wollaston.

44. This result accorded so closely with that obtained by Dr. Wollaston himself from the analysis of two other samples of bottom-water taken up by Admiral Smyth, the one in Long. 1° 0' E. from a depth of 400 fathoms, and the other in Long. 4° 30' E. from a depth of 450 fathoms,—as well as with our own determinations of the Specific Gravities and Chlorine percentages of a great number of samples taken in different parts of the Western basin of the Mediterranean,—that we cannot hesitate in regarding it as representing the *ordinary* condition of the bottom-water at this spot. And it seems to us far more probable that the sample furnished by Admiral Smyth to Dr. Wollaston had been concentrated by evaporation in a badly stopped bottle, in the three years during which it had remained in Admiral Smyth's possession, than that any extraordinary discharge of salt from a brine-spring at the bottom (a sort of *Deus ex machina* invoked by Admiral Smyth to account for the occurrence) should have given rise, in the spot at which his Sounding was taken, to an exceptional condition of which no indication whatever was presented in our own.

45. The Temperature-phenomena presented at this Station proved of singular interest. The *surface*-temperature, 74°·5, was higher than any that had been encountered on the Atlantic side of the Straits, even in a latitude half a degree further south; and the observations, which had been regularly taken every two hours, showed that it had increased nearly *ten degrees* as we proceeded eastwards from Station 39, between 10 A.M. and 2 P.M. A part of this increase was doubtless due to the heating effect of the mid-day sun; but as the temperature of the air had not increased quite *six* degrees during the same time, and as it will be shown hereafter (§ 86),

* Thus Admiral Smyth states (Mediterranean, p. 159) the depth in mid-channel between Gibraltar and Ceuta to be 950 fathoms; whereas it is now known to be but little more than 500 fathoms. "A little further to the eastward," he says, "there is no bottom with 1000 fathoms of line up-and-down (*upwards of 1300 payed out*);" whereas the greatest depth as far east as Malaga Bay is now known not to exceed 750 fathoms. These errors are noticed in no invidious spirit, but merely to prevent their perpetuation. Admiral Smyth doubtless made the very best use of the means at his disposal; but a far more satisfactory method has now entirely superseded that formerly adopted.

by a comparison of the diurnal averages of the surface-temperature of the Mediterranean with those of the Atlantic, that the latter are at least four or five degrees higher than the former, it may be fairly assumed that at least half the increase was due to the passage from the colder Atlantic water of the mid-channel into the warmer water of the Mediterranean basin, the temperature of the latter being even here somewhat reduced by the inflow of the former.—The *bottom*-temperature was found to be here 55° ; and this corresponded closely with that which we had met with in the Strait (§ 32), while it was at least 5° higher than had been obtained at corresponding depths on the Atlantic side. Being desirous of determining the rate of its diminution, we took *serial* soundings at intervals of 10 fathoms down to 50, and then at 100 fathoms, with the following remarkable result :—

Surface	$74\cdot5$	$^{\circ}$	
10 fathoms	$69\cdot3$	diff. $5\cdot2$	
20	„	$65\cdot0$	diff. $4\cdot3$	
30	„	$63\cdot0$	diff. $2\cdot0$	
40	„	$61\cdot7$	diff. $1\cdot3$	
50	„	$59\cdot7$	diff. $2\cdot0$	
100	„	$55\cdot1$	diff. $4\cdot6$	
586	„ (bottom)	$55\cdot0$	diff. $0\cdot1$	

Thus there was a fall of $9^{\circ}\cdot5$ in the first 20 fathoms, of $5^{\circ}\cdot3$ in the next 30 fathoms, and of $4^{\circ}\cdot6$ in the next 50 fathoms; whilst from 100 fathoms to the bottom at 586 fathoms there was no further descent.

46. Whilst we were prosecuting these inquiries, we found ourselves surrounded—the surface of the Sea being extremely calm—by great numbers of the beautiful floating *Veilellæ*, which are occasional visitors to our own Coast, accompanied by the *Porpitæ*, which are more exclusively restricted to warmer seas. With these was a great abundance of a small species of Firoloid (*Firoloïdea hyalina*, D. Chiaje?), about 0·4 inch in length, the extreme transparence of which enabled every part of its organization to be readily studied microscopically, its Nervous System being specially distinguishable. Of this very interesting *Heteropod*, a full description will be hereafter published by Dr. Carpenter.

47. The result obtained by our first Temperature-sounding in the Mediterranean was fully borne out by that of the Temperature-soundings taken during three subsequent days, which show an extraordinary uniformity of *bottom*-temperature at depths from 162 to 845 fathoms* :—

* This uniformity, as we have since learned, had been previously observed by Capt. Spratt, in his Soundings in the Eastern Basin of the Mediterranean; but owing (it seems probable) to the want of “protection” in his Thermometers, he had set the uniform temperature too high, namely 59° . (See his ‘Travels and Researches in Crete,’ vol. ii. Appendix II.)

Station. No.	Depth, in faths.	Surface- temp.	Bottom- temp.
41	730	74·5	55·0
42	790	74·0	54·0
43	162	74·7	55·0
44	455	70·0	55·0
45	207	72·7	54·7
46	493	73·5	55·5
47	845	69·5	54·7

It will be observed that the surface-temperature varied between 69°·5 and 74°·5 ; and that whilst the highest temperatures were shown at Stations near the African Coast, the lowest presented itself between Cape de Gat and Cartagena. Now the Gibraltar inflow is very sensibly felt at Cape de Gat, where the current usually runs at the rate of a mile an hour ; and of the strength of this current we had unpleasant experience. For on the 19th of August, as we were crossing from Station 46 towards the Spanish coast, we encountered a strong N.E. breeze, which, meeting the current, worked up a considerable swell ; this prevented us from taking even a Temperature-sounding on that day, and gave our Ship a peculiar twisting or screwing movement, from which we were glad to escape by the subsidence of the breeze during the following night. During this day the Surface-temperature of the Sea came down from the average of 72°·2, which it had maintained on the 18th, to 66°·9. Had the weather been calm we might have attributed this reduction to the colder Gibraltar in-current ; but as the average temperature of the Air also fell from 73°·8 to 69°·8, and as the strong N.E. breeze must have had a cooling effect upon the surface of the sea, we should have deemed it probable that the reduction of Surface-temperature was due at least as much to the latter as to the former of these causes, had it not been that a set of Serial soundings which we took at Station 47 showed that the reduction extended very far down, as will be apparent on comparing the following results with those given in § 45 :—

	° Fahr.
Surface	69·5
10 fathoms	59·0
20 „	57·5
30 „	56·5
40 „	55·7
50 „	55·3
100 „	54·7
845 „ (bottom)	54·7

It will be seen hereafter that the observations made on our return voyage gave more distinct evidence of the cooling influence of the Gibraltar in-current (§ 86).

48. At most of these Stations we explored the bottom by means of the Dredge, with results much less profitable than we had anticipated. Except

near the Coast, on either side, where the ground was rocky and unequal, the bottom was found everywhere to consist of a tenacious mud, composed of a very fine yellowish sand mixed with a bluish clay,—the former predominating in some spots, the latter in others. Large quantities of this mud were laboriously sifted, often without yielding any thing save a few fragments of Shells, or a small number of *Foraminifera*; and in no instance was it found to contain any considerable number of living animals of any description. Our disappointment at this unexpected paucity of life was not small; and it was destined, as will hereafter appear, to continue through the whole of our Dredging-exploration of the deeper portions of the Mediterranean basin. The operation of Dredging in the shallower portions nearer shore was rendered difficult by the rocky nature of the bottom, on which the dredge continually “fouled;” and after the loss of two more dredges and a considerable quantity of rope, Capt. Calver came to the conclusion that the “tangles” only should be used where the inequality of the soundings indicated danger to the Dredge. Now the “tangles,” whilst gathering Polyzoa, Echinoderms, Crustacea, and the smaller Corals, sometimes even better than the Dredge, pick up but few Shells; and hence our collection of Mollusca is altogether a scanty one. Nevertheless many of the types we did obtain were of considerable interest. Thus at Station 45, at a depth of 207 fathoms, we got *Turbo Romettensis*, Seguenza, MS. (Sicilian fossil); *Scalaria plicosa* (Sic. foss.); *Odostomia obliquata*, Ph.; *Philine*, two undescribed species; and an interesting Coral (*Dendrophyllia corrugosa*).

49. On the afternoon of Saturday, Aug. 20th, we anchored in Cartagena Bay, in which we got *Taranis Mörchi*, Malm (northern), and *Pleurotoma decussata*, Ph. (Sic. and Cor. Cr. foss.). We left this harbour on the following Monday morning, and proceeded to a point (Station 48) at which we expected to find deeper water than any we had previously sounded in this year's work. Bottom was here struck at 1328 fathoms; and its temperature, 54°·7, proved still conformable to the uniform standard previously observed. The density of the water was not as great as we had found it on a shallower bed, its Specific Gravity being 1·0282, and its proportion of Chlorine 21·32. The specimen of the bottom brought up by the Sounding-apparatus was not promising; and when the dredge was hauled in, filled with stiff mud, but without any sign of Animal life, we experienced the truth of the beatitude “Blessed are they who expect nothing, for they shall not be disappointed.” Large quantities of this mud were washed and sifted without yielding more than a few comminuted fragments of shell; and we were reluctantly driven to the conclusion that there was “nothing in it.” The like result attended the exploration we made the next day at another Station (49), where we found the depth to be 1412 fathoms, and the Temperature and Density of the bottom-water to be almost exactly the same as at the last Station. We then steamed towards the Algerine Coast, and took a dredging in shallow water 5–51 fathoms, which gave us a few Shells of considerable interest:—*Venus multilamella*, Lamarck (Monte

Mario foss.); *Solarium pseudoperspectivum*, Brocchi (Sic. foss.); *Mitra sonata*, Marryat (Sic. foss.); *Mytilus incurvatus*, Ph. (Sic. foss.); *Sportella Cailleti*, Conti (Monte Mario foss.).

50. During the night we again steamed out into Deep water, and on the morning of Ang. 26th found at Station 45 a depth of 1415 fathoms, with a bottom-temperature of $54^{\circ}7$. The Density of the Bottom-water was almost exactly the same as that of the two previous deep-water samples. Our dredging was here more successful, the following species of Mollusca being obtained:—*Nucula*, sp. n. (*quadrata*); *N. pumila*, Asbjørnsen (northern); *Leda*, sp. n. (Portuguese also); *Pecchiolia granulata*, Seg. (Sic. foss.); *Hela tenella*, Jeffr. (northern, and Sic. foss.); *Trochus gemmulatus*, Ph. (Sic. foss.); *Rissoa subsoluta*, Aradas (Sic. foss.); *Natica affinis*, Gmelin (northern, and Sic. foss.); *Trophon multilamellosus*, Ph. (Sic. foss.); *Nassa prismatica*, Br. (Sic. foss.); *Columbella haliæti*, Jeffr. (northern, and Sic. foss.), ?=*Buccinum acuticostatum*, Ph.; *Pleurotoma carinata*, Cristofori and Jan (northern, and Sic. foss.); *P. torquata*, Ph. (Sic. foss.); *P. decussata*, Ph. (Sic. foss.); *Planorbis glaber*, Jeffr. (fresh-water!); *Spirialis physoides*, Forbes, =*S. recurvirostra*, A. Costa.

51. Directing our course again towards the Algerine Coast, we kept nearly parallel to it during the greater part of the next day, occasionally sweeping the bottom with the “tangles,” which gave us abundance of Polyzoa, Echinoderms, &c. of well known types, without any specimens of novel or peculiar interest, except (at Station 50, in 7–51 fathoms) a specimen of *Trochus biangulatus*, Eichwald, *fide* Hornes, =*T. ditropis*, S. Wood, a Miocene and Coralline Crag shell. We reached Algiers on the afternoon of the 26th; and as it was necessary to take in coal, we remained in harbour until the 29th, when we resumed our easterly course, still keeping near the Coast. The weather now began to be oppressively hot, the Surface-temperature of the Sea rising to 76° or 78° , and that of the Air being often several degrees higher. Wishing to see what would be the point at which the effect of this extreme superheating would cease to manifest itself, we took a set of Serial soundings at Station 53, with the following result, which we incline to consider typical of the condition of the proper Surface-water of the Mediterranean in the Summer season:—

	° Fahr.
Surface	77
5 fathoms	76
10 „	71
20 „	61·5
30 „	60
40 „	57·3
50 „	56·7
100 „	55·5

Thus the amount of heat lost in the first 20 fathoms is no less than $15^{\circ}5$; and as much as $9^{\circ}5$ of this loss shows itself between 10 and 20 fathoms.

Somewhat nearer the shore, at a depth of from 40 to 80 fathoms, we got the following Mollusca:—*Pecchiolia*, sp. n. (Sic. foss.) ; *Solarium pseudo-perspectivum* (Sic. foss.) ; *Nassa semistriata*, Brocchi (Sic. foss., and Coralline and Red Crag)=*N. labiosa*, S. Wood, =*N. trifasciata*, H. Adams ; and *Bulla*, sp. n.

52. Again proceeding into deep water we perseveringly explored the bottom with the Dredge ; and from a bottom of 1508 fathoms we brought up some hundredweights of the same barren mud as had previously given so much trouble and to so little profit. The sieve and the washing-tub again returned for answer “barren all.” Disappointing as this negative result was to us as Zoologists, there are aspects under which it may be viewed that may give it no small value to Geologists. On these, however, we can more fittingly enlarge hereafter (§ 102). Once more, shifting our ground a few miles, we put down our dredge in 1456 fathoms’ water, and brought it up loaded with a similar profitless freight.

53. We now determined to keep closer to the Shore, and worked for several days along the African Coast, for the most part using the “tangles,” the ground being too rocky for the dredge. Here we came upon a small fleet of Coral-fishers ; and were not a little interested in finding that they employed “tangles” similar to our own as their most effective method of collecting. We swept the shore with these very assiduously, usually between 50 and 100 fathoms ; and although we obtained Polyzoa, Echinoderms, and Corals in considerable abundance, there were not many of special interest. We may note, however, that several of the *Polyzoa* which occurred in the region in which the Red Coral is found, had, when fresh, a red colour nearly as brilliant as that by which it is characterized ; but this colour, in the Polyzoa, was quite evanescent. At Station 55 we obtained the following Mollusca:—*Leda acuminata*, Jeffr. (Sic. foss.), *Dentalium abyssorum*, Sars (northern), and *Turritella subangulata*, Brocchi (Sic. foss.).

54. The extreme heat of the weather having produced an exhausting effect upon our crew, especially on the engineers and stokers, Capt. Calver considered it desirable to give them rest ; and we accordingly made for the Bay of Tunis, which we reached at mid-day on Saturday, Sept. 3rd. The town itself is situated at the head of a shallow lagoon or salt-lake, that communicates with the sea by a narrow channel ; and at this entrance there is a small sea-port named the Goletta, having a basin for vessels of moderate size. The lake, although about six miles long, has only from six to seven feet of water at its deepest part ; and when the water is unusually low, a small Steamer, which plies between the Goletta and Tunis, is not always able to run, as happened at the time of our visit. Owing to the great evaporation, and the absence of any stream of fresh water, the water of this lake is usually very salt ; but when heavy rains fall, the level is considerably raised, and the saltiness is diminished. Thus the condition of this lake in regard to that of the sea outside is sometimes that of the Mediterranean in regard to that of the Atlantic (§ 122), and some-

times that of the Baltic towards the German Ocean (§ 123); and we would suggest whether it might not be possible, through our Consulate (which has an office at the Goletta), to have a regular series of observations made upon the relative densities of the water of the lake and that of the sea, and upon the direction of the upper and under current in the channel of communication between them, that might furnish valuable data for the complete elucidation of the subject of currents occasioned by excess of evaporation. We availed ourselves of this short rest to visit the town of Tunis, which, for the most part, retains its genuine Moorish characters; as well as the ruins of Carthage, a few miles off, the most remarkable part of which consists of a series of immense reservoirs for water, supplied by an aqueduct that brought it from a range of mountains at no great distance, from which also the modern town of Tunis is supplied.

55. Quitting Tunis at mid-day on Tuesday Sept. 6, we resumed our Dredging-explorations on more productive ground,—the shallow between the Eastern and Western basins of the Mediterranean, that extends between the African coast and Sicily, and is termed the “Adventure Bank,” from having been first discovered by Admiral Smyth when surveying in H.M.S. ‘Adventure.’ The depths here range from about 30 to 250 fathoms. When passing Cape Bon, we fell in with another small fleet of Italian Coral-fishers; and were surprised at the large outlay incurred for such small returns as they seemed to be obtaining. We here found, between 25 and 85 fathoms, the following species of MOLLUSCA: *Trochus suturalis*, Ph. (Sic. foss.); *Xenophora crispa*, König (Sic. foss.); *Cylichna striatula*, Forb. (Sic. foss.); *C. ovulata*, Brocchi (Sic. foss.). And seven miles off the point called Rinaldo’s Chair, between 60 and 160 fathoms, we obtained *Tellina compressa*, Brocchi (Sic. foss.), a species possessing the following synonyms—*Tellina striatula*, Calcare; *T. strigilata*, Ph.; *Psammobia Weinkauffi*, Crosse; and *Angulus Macandrei*, Sowerby: also an interesting ANNELID, *Praxilla prætermissa*, Malmgren (northern). Here, again, we brought up a great abundance of *Polyzoa*; and many of these have proved of great interest. One, in particular, of a beautiful, very open reticular plan of growth, is the type of a new genus, of which another species had been previously obtained by Mr. Busk from the Canary Islands, and which he will describe under the name of *Climacopora*. Many of the species obtained had been previously known only as Tertiary Fossils. A complete account of them will be published by Mr. Busk hereafter. Abundance of Shells were here found; among them we obtained a considerable number of living specimens of *Megerlia truncata*, including a whole series in various stages of growth, the youngest of which presented a very remarkable character,—a set of *setæ* projecting from the margin of the shell, the length of which exceeded its own long diameter. Among other species of interest were:—*Kellia*, sp. n. (Sic. foss.); *Gadinia excentrica*, Tiberi; *Rissoa*, sp. n.; *Scalaria frondosa*, J. Sow. (Sic. & Cor. Cr. foss.); *Odostomia unifasciata*, Forbes; *Pyramidella plicosa*, Bronn (Sic. & Cor. Cr. foss.)=*P. læviuscula*, S. Wood; and *Actæon pu-*

sillus, Forb. (Sic. foss.). Among the *Annelida* was an interesting Northern form, *Hyalinæcia tubicola*, Müller. The *Echinodermata* were in considerable abundance, but were mostly well-known Mediterranean types. The *Cidaris hystrix* was especially frequent; and a comparison of the series of specimens obtained in this and the preceding Cruise, with those obtained last year in the Northern area, has enabled Prof. Wyville Thomson to satisfy himself that *C. hystrix*, *C. papillata*, and *C. affinis* are specifically identical. The *Corals* found on the Adventure Bank have proved peculiarly interesting; and will be the subject of a special Report by Prof. Duncan. Among the *Hydrozoa* were two undescribed species of *Aglaophenia*. Although no new *Foraminifera* here presented themselves, yet it was very interesting to obtain (as we had previously done at several points on the African coast) specimens of the beautiful *Orbitolites tenuissimus* first discovered last year in the Atlantic (Report, § 36), of which we had ventured to predicate the existence in the Mediterranean from having discovered an extremely minute fragment of it in one of Captain Spratt's *Ægean* dredgings; also peculiarly large and elaborately finished specimens of the great Nautiloid *Lituola*, first met with last year, the "test" of which is built up of sand-grains united by a ferruginous cement, with passages extending from the principal chambers into their walls, forming what is known as the "labyrinthic structure," of which the greatest development is found in the two gigantic Fossil types (*Parkeria* and *Loftusia*) recently described by Dr. Carpenter and Mr. H. Brady*.

56. This part of our work having brought us to the neighbourhood of the Island of Pantellaria, we landed on it with the view of visiting, if possible, a cavern which had the reputation of being "of icy coldness." As we found, however, that a whole day's delay would be involved, we gave up the idea; and we afterwards obtained elsewhere the information we desired (§ 89).—The continuance of the very hot weather having brought a large part of our Crew to a state of such exhaustion as to render a continuation of our operations undesirable, Captain Calver considered it expedient to proceed to Malta without further delay; and we anchored in the Harbour of Valetta on the morning of Saturday, Sept. 10th. Here we found it necessary to remain for ten days, the illness of our Chief Engineer, which we at first hoped might be only temporary, proving sufficiently serious to require that a substitute should be found for him. Our time was passed very pleasantly in visits to the various objects of interest in which the Island abounds, and in the enjoyment of the kind hospitality of His Excellency the Governor, Vice-Admiral Key, and other Officers.—The time was too short for any careful examination of the Geology of the Island; but one point which struck us as of special interest in relation to the deposit at present forming on the Mediterranean bottom will be specially noticed hereafter (§ 102).

57. As the instructions which we received at Malta required us to

* Philosophical Transactions, 1869.

proceed homewards without unnecessary delay, and as Capt. Calver was desirous of avoiding, if possible, the necessity of going into port for coal between Malta and Gibraltar, we found ourselves obliged to relinquish the hope we had entertained of being able to resume our Dredging-explorations in deep water along a different line on our return voyage. But, desiring to gain what addition we could to the information already acquired respecting the Physical condition of the Mediterranean, we arranged so to shape our course on leaving Malta as to enable us (1) to obtain a deep Sounding in the Eastern basin, and (2) to ascertain the Bottom-temperature on an area of Volcanic activity.

58. Quitting Valetta Harbour at mid-day on September 20th, we steered in a N.E. direction towards a point about 70 miles distant, at which a depth of 1700 fathoms was marked on the Chart. This we reached early the next morning (Station 60); and a Sounding being taken, 1743 fathoms of line ran out. As this was the greatest depth we had anywhere met with in the Mediterranean, and as the Basin in which the Sounding was taken is cut off by the shallows between Sicily and Tunis from all but superficial communication with the Western basin, we watched the heaving in of the Sounding-apparatus and its accompaniments with no little interest. The Thermometers recorded a Temperature of 56° , which was one degree *higher* than that which we had met with in our two deepest Soundings (1456 and 1508 fathoms) in the Western basin. The sample of the bottom brought up in the tube of the Sounding-apparatus indicated the prevalence of a yellowish clayey deposit so similar to that which had elsewhere proved so disappointing, that we could not feel justified in pressing Capt. Calver for the sacrifice of nearly a whole day, which would have been required for a single cast of the Dredge at this depth. The specimen of Bottom-water brought up by our Water-bottle surprised us by its very small excess of Density above the Surface-water; the Specific Gravity of the former being only 1.0283, whilst that of the latter was 1.0281; and the proportion of Chlorine per 1000 being 21.08 in the former, whilst that of the latter was 20.77. The Surface-water being here *more* dense than the average, the Bottom-water was *less* dense—a result which a good deal surprised us at the time, but which subsequent comparison with the densities of specimens taken from the greatest depths we had sounded in the Western basin (§ 93) showed to be by no means exceptional; and when we came to reason out the mode in which surface-evaporation may be presumed to operate in augmenting the density of the water beneath, we found it to be quite in accordance with *a priori* probability, that the *deepest* water should show the *least* excess of density above the water at its surface (§ 94).

59. Having thus satisfied ourselves, so far as we could do by a single set of observations, that the Physical conditions which we had found to prevail in the Western basin of the Mediterranean present themselves also in the Eastern, we steered for the Coast of Sicily; and in a few hours

came in sight of Syracuse, with the lofty mass of Etna as a magnificent background in the remote distance. The clouds which lay upon its summit during the earlier part of the day gradually dispersed as we approached it, so that we could distinctly trace the outline of its cone, save where this was obscured by a constantly shifting semitransparent cloud. Whether this was a light smoke given off from the crater, or a film of vapour condensed by the contact of a current of warm moist air with the colder surface of the mountain-summit, we were unable to distinguish, though we watched it with great interest during the whole afternoon.—We steamed quietly along the Sicilian coast during the night, so that sunrise the next morning found us in the narrowest part of the Strait of Messina, between Messina and Reggio; and we shall not easily forget the beauty of the spectacle we then beheld on either shore. Passing through the once-dreaded Charybdis, the dangers of which are rather poetical than real, and leaving on our right the picturesque castle-crowned rock of Scylla, we passed out of the “Faro,” which narrows at its northernmost extremity to about $3\frac{1}{2}$ miles, into the open sea to the North of Sicily, studded by the Lipari Isles, and steered direct for Stromboli, stopping at 10 A.M. to take a Sounding (Station 61). This gave us a depth of 392 fathoms, and a Bottom-temperature of $55^{\circ}\cdot7$, which afforded no indication of unusual elevation. Here again we found the Density of the Bottom-water scarcely in excess of that of the Surface-water; and it was even lower than that of the Surface-water in another Sounding taken somewhat further on (Station 62), and at the depth of 730 fathoms, which gave a Bottom-temperature of $55^{\circ}\cdot3$.

	Sp. Grav.	Chlorine.
Surface.	1·0281	21·32
Bottom, 392 fathoms	1·0282	21·36
Bottom, 730 fathoms	1·0280	21·22

60. This result, again, surprised us much at the time; but we are now inclined to attribute it to the decrease of surface-evaporation consequent upon the marked decrease in the heating-power of the Sun, which showed itself in the change of the relative Temperatures of the Sea and Air; for whilst, for some days before we put into Malta, the Surface-temperature of the Sea had ranged between 76° and 78° , and the Temperature of the Air had been usually about 1° *higher*, we now found that while the Surface-temperature of the Sea ranged between $73^{\circ}\cdot6$ and $76^{\circ}\cdot6$, the Temperature of the Air was between 2° and 4° *lower*. This difference continued to show itself nearly all the way to Gibraltar; the daily averages of the Surface-temperature of the Sea ranging between $73^{\circ}\cdot1$ and $75^{\circ}\cdot6$, whilst those of the temperature of the Air ranged between $68^{\circ}\cdot5$ and 72° . We now approached the rugged cone of Stromboli, from the summit of which there was constantly issuing,—as has been the case since the time when the neighbouring island of Hiera was fabled to be the workshop of Vulcan,—a cloud

of smoke, indicative of active changes in the molten depths beneath. Of this activity, however, we had found no special indication in the Temperature-soundings taken nearest to the island. Whether the *general prevalence* in the neighbourhood of Sicily of a Bottom-temperature averaging about a degree above that of the Western part of the Mediterranean is due to Subterranean heat, is a question which can only be determined by a larger number of observations than we had the opportunity of making. As we neared Stromboli, we were much struck with the height to which the energetic industry of its inhabitants had carried the vine-cultivation all round the cone, save on two slopes looking N.W. and S.E., over one or other of which there is a continual discharge of volcanic dust and ashes. Although no flames were visible during daylight, we could distinctly perceive occasional flashes as night came on.—Our course was now laid straight for Cape de Gat, which we passed on the 27th of September, arriving at Gibraltar on the evening of the 28th. The only Scientific observations which we had the opportunity of making during this part of our voyage were confirmatory of those which we had made at the commencement of our Mediterranean Cruise, as to the lower Temperature and inferior Density of the Surface-water, both which we attribute to the inflow from the Atlantic. (See §§ 86, 92).

61. Having taken in at Gibraltar as much coal as we could carry, we left the harbour at 9 A.M. on the 30th Sept., and proceeded at once towards the scene of our previous observations. We thought it worth while, however, to take a Sounding in our way towards this, near the 100-fathom line (Station 63), for the sake of ascertaining the Temperature and Specific Gravity of the bottom-water. The depth was found to be 181 fathoms, showing that the slope from the shallow to the deep portion of the channel is here very rapid. The bottom-temperature was $54^{\circ}\cdot7$, that of the surface being 68° ; and the Specific Gravity of the bottom-water was 1.0280, that of the surface being 1.0271. This bottom-water thus agreed closely in both particulars with that of the *deep* mid-channel, as ascertained in our first set of observations (§ 34), which were afterwards confirmed by our second. We then steamed out to a point (Station 64) nearly identical with that from which our previous investigations had been carried on; and commenced our work with a Temperature-sounding. The surface-temperature ($65^{\circ}\cdot6$) proved to be here *less* by $2^{\circ}\cdot4$ than it had been found to be at Station 63; and this although it was taken an hour later in the forenoon, when an increase might have been expected. It thus corresponded closely with what had been previously found to be the average temperature of the Strait in mid-channel, both during the first approach to Gibraltar from the westwards (§ 30), and during our own experiments at the commencement of the Mediterranean Cruise (§ 32); and the continuation of the like observations during the remainder of the day and ensuing night (§ 65) gave the same remarkable result, the *rationale* of which will be considered hereafter (§ 74). The depth was somewhat less than at the neighbouring Station 39,

being 460 fathoms instead of 517 ; but the bottom-temperature was a little lower, being $54^{\circ}\cdot7$ instead of $55^{\circ}\cdot5$. The respective Specific Gravities of the Surface- and Bottom-waters, and of that of the Intermediate stratum of 250 fathoms, were found to coincide almost exactly with those previously determined, as the following comparative statement shows :—

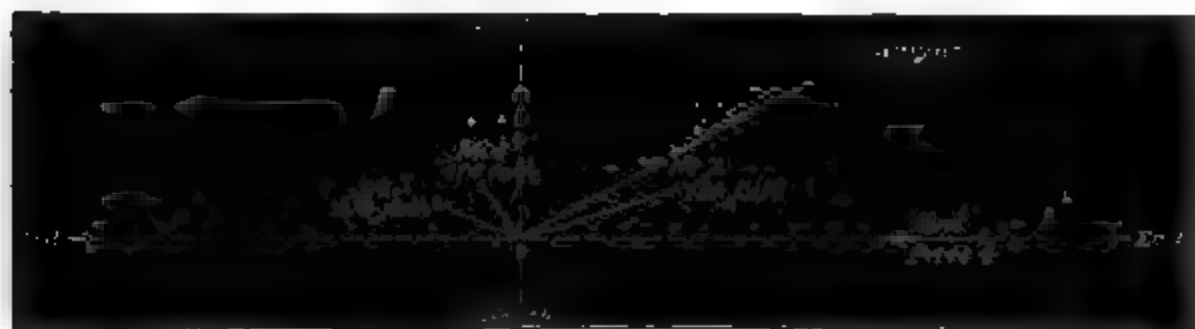
	Specific Gravity. Station 39.	Specific Gravity. Station 64.
Surface	1027·1	1027·1
250 fathoms	1029·3	1029·2
Bottom	1028·1	1028·3

Now the density of the *bottom*-water here corresponds so exactly with that which prevails over the *deeper* bottom of the Western basin of the Mediterranean, whilst it so considerably exceeds that of the *bottom*- as well as of the *surface*-water of the Atlantic, that we cannot fail to recognise it as belonging to the Mediterranean basin ; so that, if it has any motion at all, we should expect that motion to be from east to west. Still more certainly may this be affirmed of the *intermediate* stratum, the density of which corresponds with that of the bottom-water of the *shallower* part of the Mediterranean basin ; the greatest depth (586 fathoms) at which such water was obtained being at Station 40, the nearest point to the Strait from which a specimen of bottom-water was obtained (§ 43). And it may be further predicated that a stratum of water of a density of 1029·3 could not overlies water of the density of 1028·1, unless it *moved over* the stratum below,—that is, unless (1) the two strata were moving in opposite directions, or (2) were moving at different rates in the same direction, or (3) the upper stratum were in motion in either direction, and the lower stratum were stationary. It will presently appear that the second of these conditions is the one which obtains in the present case.

62. We now proceeded to repeat our experiments with the “current-drag,” with the view of obtaining, if possible, unequivocal evidence of the existence of that Westerly undercurrent which so many considerations combined to render probable.—The direction of the Wind during this set of experiments was from the East, or opposite to that of the surface-current ; and its force (3 to 4) was sufficient, by its *meeting* the current, to produce a considerable swell, which necessitated the employment of a larger boat, and rendered it unsafe to allow her to drift without men. The sectional area of the boat was therefore greater than on the former occasion, giving the in-current a stronger hold upon her ; but, on the other hand, the surface she presented to the wind was also greater ; and as this acted in the opposite direction, the latter increase might be considered to neutralise the former, or even rather to exceed it, so as to render the boat more capable of being carried westwards by the “current-drag,” if this should be acted on by an outward undercurrent. The rate of surface-movement was tested as before (§ 36), and proved to be 1·823 mile per hour, its direction

being N.E. by E. $\frac{1}{2}$ E. This was a retardation of more than a mile per hour, as compared with the former observation; and that it was not attributable to the mere surface-action of the easterly wind, was clear from the result of the next observation, which showed that the retardation extended to a depth far below the influence of surface-action.—The “current-drag” having been lowered to 100 fathoms’ depth, the drift of the boat was reduced to 0·857 mile per hour, or *less than half* its surface-drift; its direction was nearly the same as that of the surface-current, viz. E. by N. $\frac{1}{2}$ N. The “current-drag” was then lowered to a depth of 250 fathoms; and in a short time the boat was seen to be carried along by it in a direction (W.N.W.) *almost exactly opposite* to that of the middle in-current of the Strait. The rate of outward movement of the boat was 0·400 mile per

Station 64.



Rate (per hour) and Direction of Movement of Surface-float and of Current-drag at different Depths; with Force and Direction of Wind.

hour; but from the considerations formerly stated (§ 39), it is clear that the actual rate of the undercurrent must have exceeded that of the boat on the surface.—The “current-drag” was then lowered down to a depth of 400 fathoms; and again the boat was carried along in nearly the same direction as in the previous experiments, namely N.W. $\frac{1}{2}$ N.; but more slowly, its rate of movement being 0·300 mile per hour.

63. Thus, then, our previous deductions were now justified by a *conclusive proof* that there was at this time a return-current in the mid-channel of this *narrowest* part of the Strait, from the Mediterranean towards the Atlantic, flowing beneath the constant surface-stream from the Atlantic into the Mediterranean; and it will be shown hereafter (§ 115), by a comparison of all the results of our observations, that a strong presumption may be fairly raised for the *constant* existence of such a return-current, though its force and amount are liable to variation.

64. As the determination of the boundaries of this return-current, and of the amount and conditions of its variation, could only be effected by multiplied simultaneous observations at different points, with ample license as to time, neither of which fell within the scope of the present Expedition, we were obliged to content ourselves, as regards this locality, with what we had found ourselves able to accomplish; and at the conclusion of this day’s work we proceeded westwards under easy steam, so as to be able

to resume our experiments the next morning in the *shallowest* part of the Strait.

65. The average Surface-temperature of the Mid-stream during our outward passage through the Strait proved to be 66° , thus corresponding exactly with what we had found it to be on our inward passage seven weeks previously (§ 30). This depression, as compared with the surface-temperature of the Strait itself nearer the shore, both north and south, and with the temperature of the Mediterranean to the eastward and that of the Atlantic to the westward, is extremely remarkable. We shall hereafter inquire how it is to be explained (§ 74).

66. The breadth of the Channel between Capes Spartel and Trafalgar (Chart II.) is about 23 nautical or $26\frac{1}{2}$ statute miles. Its northern half is much shallower than the southern, as is shown in Section B B; the 100-fathom line off the Spanish coast running at about twelve miles' distance from Cape Trafalgar, whilst along the African coast it keeps much nearer the shore, being at only two miles' distance from Cape Spartel. Between these two lines, the greatest depth marked in the Chart is 194 fathoms; and this occurs off Cape Spartel, at less than a mile from the 100-fathom line. Between this and the opposite border of the deeper channel, the depths vary from 130 to 180 fathoms; the abruptness of the differences at neighbouring points indicating a rocky bottom, of which we soon had unpleasant experience.

Stations 65, 66.



Rate (per hour) and Direction of Movement of Surface-float and of Current-drag at different Depths; with Force and Direction of Wind in No. 3. (No Wind in Nos. 1, 2.)

67. We commenced our observations on the morning of Oct. 1st at the point of greatest depth (Station 65). The temperature of the surface at 6 A.M. was only 63° , which was at least eight degrees lower than the average temperature at that hour within the Mediterranean. The bottom-temperature at 199 fathoms was $54^{\circ}\cdot 5$, and the Specific Gravity of the bottom-water was 1028·2. The coincidence both in temperature and Sp. Gr. with the bottom-water at Station 64 was thus very close. The place of

'the Ship having been determined by angles taken with the shore, the rate of the surface-movement was tested as on former occasions, and was found to be 1·277 mile per hour, its direction being E. $\frac{1}{2}$ S. The "current-drag" was then sunk to 150 fathoms, the greatest depth at which it was thought safe to use it; and the boat from which it was suspended moved E. $\frac{3}{4}$ N. at the rate of 0·840 mile per hour. This observation indicated a very considerable retardation in the rate of inflow, but gave no evidence of an outflow. It did not, however, negative the inference deducible from the Temperature, and still more from the Specific Gravity, of the water beneath, that an outflow takes place in that lowest stratum which we could not test by the "current-drag."

68. We then steamed across the deep channel towards the Spanish side; and passing a bank of 45 fathoms which rises near its middle, we sounded again at Station 66, about six miles to the northward of Station 65. The surface-temperature at 9 A.M. was here found to have risen to 69°; and since not more than half this increase could be attributed, according to our general experience, to the increase of direct solar radiation at this period of the day, the cause of the additional elevation has to be sought elsewhere (§ 78). The length of sounding-line run out was 147 fathoms; but on attempting to reel it in, the lead was found to have fixed itself between rocks; and all Capt. Calver's skill in the management of his ship proved inadequate to free it. As we were thus anchored by our sounding-line, it was requisite to set *ourselves* free, by putting a breaking strain upon it; and we thus lost, with the lead, one of our Water-bottles, and a pair of Thermometers, one of which was specially valued by us as having been used throughout the 'Porcupine' Expedition of 1869, in which the Temperature-soundings had proved of peculiar importance. The "current-drag" was here let down to 100 fathoms; and the boat from which it was suspended moved along in the direction of the surface-current, and at the rate of 1·280 mile per hour, which was almost precisely that of the surface-current in the previous observation.

69. Deeming it important to obtain the Temperature and Specific Gravity of the bottom-water on the Spanish side of the deeper portion of the channel, we slightly shifted our ground, and again let down our lead, with Thermometers and Water-bottle, at Station 67, where the depth proved to be 188 fathoms. On beginning to reel in the line, we found the lead to have anchored as before, and for some time feared that we should sustain a second loss of the Water-bottle and Thermometers attached to it. The means taken by Capt. Calver for its extrication, however, proved on this occasion successful; and we had the satisfaction of seeing the whole apparatus safely brought up,—the lead bearing evident marks of having been jammed between rocks and then violently strained. The Temperature of the bottom proved to be 55°·3, that of the surface being 73°; and the Specific Gravity of the bottom-water was 1028·1, that of the surface being 1026·8. Here again, therefore, the evidence afforded by the Tem-

perature and Specific Gravity of the *bottom*-water was conclusive as to its Mediterranean character. Its Density corresponded rather with that of the *bottom*-water than with that of the *intermediate* stratum, at the opposite end of the Strait; but the more rapid Westerly motion of the latter (§ 62) would seem to indicate that the water which here flows over the "ridge" is derived from it, rather than from the deeper layer, and that its diminution in density is due to the dilution it sustains in its course. In either case the denser Mediterranean water discharged by this undercurrent must flow *up-hill*; but the incline is so gradual that a very small force, if constantly sustained, would suffice to produce the elevation needed to carry it over the ridge.

70. Whilst we were prosecuting these inquiries, our attention was attracted by the long chains of *Aggregate Salpæ* which were floating close to the Ship near the surface of the very calm sea. We were able to collect four or five different species of these, and to submit them, during life, to Microscopic examination. The reversal of the direction of the Circulation took place in all at more regular intervals than we have usually found to be the case in the Compound Ascidians; and we were able to distinguish an unmistakable rudimentary *eye*, which had not, we believe, been previously noticed. We hope to be able hereafter, by the detailed study of these specimens, to make some additions to the knowledge previously acquired of this very interesting group.—As the nature of the bottom put it out of the question to attempt to dredge on this ridge, our only means of investigating its Zoology lay in the use of the "hempen tangles." A "sweep" taken with these brought up a few Echinoderms and Polyzoa of no special interest; but with these there was a new species of *Amphihelia*, allied to *A. oculata*.

71. We now took our final leave of the Mediterranean basin with mingled feelings of disappointment and satisfaction. The Zoological results of our Cruise had been by no means equal to our expectations; but, on the other hand, we could console ourselves with the belief that our determination of the peculiar Physical conditions of this great Inland Sea, and in particular our elucidation of the mystery of the Gibraltar current, would be fairly regarded as a success; and we venture to think that this will be admitted by such as may follow us through the discussion of General Results to which we shall presently proceed.

72. As Capt. Calver considered himself bound not to make any unnecessary delay in returning homewards, and to take every advantage of the continuance of the fair weather and favourable breeze which we enjoyed during nearly the whole remainder of our voyage, we were reluctantly compelled to give up the idea of prosecuting any further researches in the Deep Sea; and devoted ourselves to the examination of the specimens previously collected, and to the correlation of our Temperature and other results,—specially directing our attention, however, to the Surface-temperature of the *embouchure* of the Strait, with the view of ascertaining whether a sudden *fall* would be observable on quitting it, corresponding to the *rise* which had

noticed on the outward voyage on entering it (§ 30). This change was to be very decided. As we kept along the Southern Coast of Portugal towards Cape St. Vincent, the Surface-temperature averaged $73^{\circ}\cdot5$. At 5 P.M. we were turning the corner of the Cape, and found the Surface-temperature $72^{\circ}\cdot5$; and at 8 P.M., when we were fairly in the Atlantic, we found that the Surface-temperature had fallen to 69° , thus showing a difference of $4^{\circ}\cdot5$. On the following day, when we were off Lisbon, the Surface-temperature was $69^{\circ}\cdot5$; and it gradually diminished as we proceeded southwards from that point.—Although the season of the year led us to expect a rough passage across the Bay of Biscay, the weather continued remarkably fine until we reached the “Chops of the Channel,” where we were met with a rather fresh breeze; this did not interfere, however, with our anchoring at Cowes on the afternoon of the next day (Oct. 8th), after an absence of just two months, during which a greater number of most important Public events had occurred than had ever before been crowded in so short a period.

GENERAL RESULTS.

TEMPERATURE AND COMPOSITION OF ATLANTIC WATER.

For this portion of the Report Dr. Carpenter is alone responsible.]

3. *Surface-Temperature.*—The Temperature of the surface-water at Chops of the Channel (Stations 1–9) averaged 62° for five days; and we gradually in conformity with the Southing, until at Cape St. Vincent stood at 69° . The Temperature of the *Air*, which averaged $63^{\circ}\cdot4$ in the former locality, rose to about 69° in the latter; but it is specially noteworthy that whilst, as we crossed the Bay of Biscay and drew southwards along the coasts of Spain and Portugal, the temperature of the Air was not always higher by from 2° to 5° than that of the Sea, this difference ceased to show itself as we neared Cape St. Vincent, and was even replaced by a slight difference in the contrary direction. The excess in the Surface-temperature of the Sea above the temperature of the Air became still more marked after we had passed the Cape, and had changed our course to the north; a *sudden rise* of from 2° to 4° then showing itself in the former, whilst the latter did not rise by more than half that amount. Thus on Oct. 30, between Stations 27 and 28, on our way to Cadiz, in about $36\frac{1}{2}^{\circ}$ N. and Long. $7\frac{1}{2}^{\circ}$ W., the surface-temperature of the Sea exceeded that of the Air by $4^{\circ}\cdot2$, whilst the temperature of the Air was only 72° . The like condition repeated itself after leaving Cadiz, on August 2, between Stations 29 and 30; the surface-temperature of the Sea being $73^{\circ}\cdot2$, whilst the temperature of the Air was $71^{\circ}\cdot4$. That this excess did not depend upon a reduction of evaporation, consequent upon a peculiarly damp condition of the atmosphere, appeared from the fact that the Wet-bulb thermometer during this period stood at from 3° to 4° below the Dry a difference fully equal to

the general average of our observations.—Having thence crossed the *embouchure* of the Strait of Gibraltar, so as to approach the coast of Africa, the three following days were passed in Latitudes averaging about a degree further *south* than those in which the previous observations had been noted ; yet the surface-temperature of the Sea then *fell* again to an average of 72° , whilst that of the Air averaged $73^{\circ}\cdot7$, thus nearly restoring the usual ratio between the two.—It was not a little perplexing to find, when we had fairly entered the Strait and were proceeding along the *mid-channel* towards Gibraltar, that the surface-temperature of the Sea *fell* still further to $66^{\circ}\cdot4$, whilst the temperature of the Air *rose* to $76^{\circ}\cdot6$, thus showing the then unprecedented difference of $10^{\circ}\cdot2$ between the two.

74. These remarkable phenomena caused us to give particular attention to the Surface-temperature in the *mid-stream* of the Strait, and on the northern side of its *embouchure*, on our return voyage ; when our first series of observations derived full confirmation from another series taken with the greatest care nearly two months afterwards. For when we left Gibraltar Harbour on the morning of September 30th, we found, on proceeding into the mid-stream, that the surface-temperature *fell* from $70^{\circ}\cdot7$ to $65^{\circ}\cdot6$, although the latter observation was made towards noon ; that it remained at nearly the same point through the remainder of that day and the succeeding night, during which we were slowly proceeding eastwards,—still in the mid-current ; and that it stood as low as 63° at six o'clock on the following morning (Oct. 1), when we had reached Station 65, in the deepest part of the channel over the ridge not far from the African coast. Having thence moved towards the Spanish coast, we found the surface-temperature at Station 66 to have risen to 69° ; and a little further, at Station 67, it was found to have risen to 73° .—These observations make it clear that in the line of the strongest surface-inset, the temperature of the current is *several degrees lower* than that of the surface-water of the Atlantic from which it is directly derived ; and the fact would seem to indicate either that the water of which this current consists is drawn from a part of the Atlantic at least as far north as Lisbon, or (which may be thought more probable) that it is derived from a stratum of the neighbouring ocean *somewhat beneath the surface*, so as to have received less of the solar superheating than the actual surface-water.—It would be a matter of much interest to trace this colder current to its source, and thus to ascertain how it makes its way through a sea at least five degrees warmer than itself.

75. Our second series of observations upon the Surface-temperature of the Northern side of the wide *embouchure* of the Strait were also quite confirmatory of the inference to which the first seemed to point,—that there is a *surface-outflow* of Mediterranean water along the Spanish coast, by which the Temperature is kept up above the ordinary standard of Atlantic water in that latitude ; for during the remainder of Oct. 1, the following night, and the greater part of the next day, the surface-temperature was between 70° and $72^{\circ}\cdot5$, being a degree or two *higher* than the temperature

of the Air : at 4 P.M. it was $73^{\circ}5$; at 6 P.M., when we were passing Cape St. Vincent, it was $72^{\circ}5$; and at 8 P.M., when we were fairly in the Atlantic, it was at 69° . The average of the next two days, while we were proceeding steadily Northwards, was maintained at nearly the same point ; and the surface-temperature of the Sea was now pretty constantly *lower* than that of the Air.

76. The most probable explanation of this excess seems to lie in the fact that, besides the mid-current almost invariably setting eastwards, there are two lateral streams, of which the direction is sometimes reversed under tidal influences (§ 106) ; and that warmer water from the Mediterranean basin thus finds its way outwards, chiefly along the Spanish shore.—That the surface *indraught* is greatest on the African side of the Strait, and that the surface *outflow* is greatest on the Spanish side, is, we understand, a fact well known to those who are in the habit of navigating it, though we do not find it mentioned in the ‘Sailing Directions ;’ and this is just what our observations of surface-temperature in the *embouchure* of the Strait would lead us to infer.

77. It is a circumstance worthy of remark, that an abnormally low temperature showed itself during the whole of our first stay in Gibraltar Bay, from Aug. 7 to Aug. 14 ; the surface-temperature of the Water ranging between $64^{\circ}1$ and $65^{\circ}6$,—being apparently that of the mid-strait to the eastward,—whilst the temperature of the Air ranged between $72^{\circ}9$ and $77^{\circ}2$, the greatest difference between the two being $12^{\circ}5$. During most of this time the wind was easterly, and the wet-bulb thermometer ranged from $4^{\circ}1$ to $8^{\circ}2$ below the dry ; but the increased evaporation that would result from the atmospheric condition thus indicated, could scarcely have produced the marked depression observable in the surface-temperature of the water ; more especially as our general experience was that the surface-temperature in the comparatively shallow water of a Harbour was rather higher than in the deeper sea outside.—No such depression presented itself on our return voyage. On approaching Gibraltar from the Mediterranean side, there was a gradual reduction from 74° , which had been the average of several previous days, to 72° , apparently in consequence of the influx of the colder Atlantic water ; the water in the Bay itself had an average surface-temperature of nearly 71° ; and the surface-temperature did not fall until we came out into the mid-channel, where we encountered the colder *indraught* as already described (§ 61). Hence the depression observed during our first visit must be occasional only ; and may perhaps be attributable to a deflexion of the mid-current into the Bay, by the opposing influence of the easterly wind which then prevailed.—This is one of the points as to which further inquiries, such as may be easily instituted by the Government authorities at Gibraltar, would doubtless furnish information of great interest.

78. *Bottom-Temperature*.—The Temperature-soundings taken during the Atlantic Cruise (see p. 220) may be arranged in three sets :—

I. Those taken in the “Chops of the Channel,” about Lat. 48° N., corresponding closely in geographical position with several of those of the Second Cruise of 1869.

II. Those taken off the Atlantic Coast of Spain and Portugal, between Lat. 42½° N. and Lat. 37° N.

III. Those taken within the *embouchure* of the Strait of Gibraltar, extending westerly as far as Cape St. Vincent and Tangier, and lying between Lat. 37° N. and 35½° N.

79. The first two sets may be advantageously compared with each other, and with a Set of Bottom and Serial (No. 42) Soundings taken last year nearly in the same locality as the first; these are presented in the following Table.

Temperature of the Sea at different Depths near the Western margin of the North-Atlantic Basin.

I. Chops of the Channel, 1869.				II. Chops of the Channel, 1870.				III. Coast of Spain and Portugal			
No.	Depth, in fathoms.	Surface- Temp. ° Fahr.	Bottom- Temp. ° Fahr.	No.	Depth, in fathoms.	Surface- Temp. ° Fahr.	Bottom- Temp. ° Fahr.	No.	Depth, in fathoms.	Surface- Temp. ° Fahr.	Bottom- Temp. ° Fahr.
34.	75	66·0	49·7	7.	93	61·0	51·3	10.	81	60·5	53·5
35.	96	63·4	51·3	5.	100	62·3	51·5	12.	128	61·5	52·5
42.	250	62·6	50·2	8.	257	60·7	50·0	19.	248	64·7	51·7
42.	300	62·6	49·6	2.	305	61·5	48·7	11.	332	60·5	51·5
42.	350	62·6	49·1	6.	358	62·0	50·3	17.	340	67·0	50·5
42.	450	62·6	47·6					14.	469	69·7	51·5
39.	557	63·0	47·0	9.	539	64·0	48·0				
42.	600	62·6	45·5					21.	620	67·3	50·5
36.	725	63·9	43·9	4.	717	61·5	45·5	22.	718	66·5	50·5
								15.	722	67·5	49·7
42.	750	62·6	42·5					17a.	740	66·7	49·0
42.	800	62·6	42·0					23.	802	66·5	49·3
42.	862	62·6	39·7					16.	994	69·5	40·3
38.	1000	64·0	38·3					18.	1065	65·0	39·7
38.	1250	64·0	37·7					17.	1095	68·0	39·7

Between Nos. I. and II., as might be anticipated, the accordance is extremely close; and this accordance extends to the upper stratum (excluding the actual surface) in No. III., allowance being made for difference of Latitude. Notwithstanding that the *surface*-temperatures in No. III. range as high as 69°·7, the average excess at depths between 81 fathoms (at which the *superheating* of the surface has but little effect, § 87) and 350 fathoms is not above 2°, the reduction of temperature encountered in descending this upper stratum being very small in each case. But whilst this slow rate of reduction continues in No. III. down to 800 fathoms,—the bottom-temperature at 802 fathoms being 49°·3,—the reduction is more rapid in Nos. I. and II., so that the temperature of 45°·5 is reached in the one at 600 fathoms, and in the other at 717; whilst at 800 fathoms in No. I. the temperature has fallen to 42°. Below 800 fathoms, how-

ever, in No. III., the temperature undergoes so extraordinarily rapid a depression, that it is reduced *nine degrees* within the next 200 fathoms; the water at 994 fathoms being only 2° warmer than it is at 1000 fathoms in No. I. A slight further reduction of temperature is noticeable in the two deepest Soundings taken in the Atlantic Cruise of 1870, the temperature found at nearly 1100 fathoms being just 2° higher than that found at 1250 in No. I.

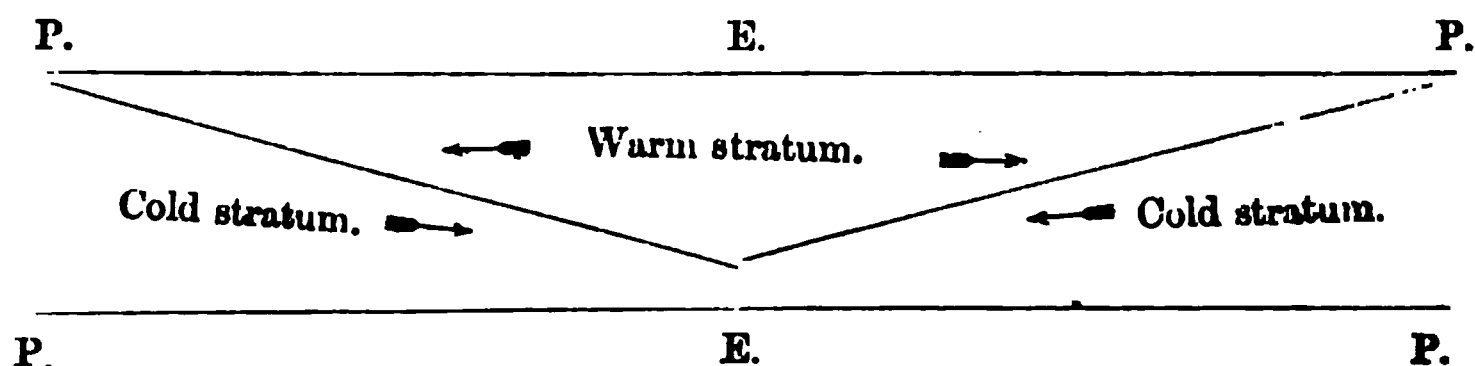
80. Considering these facts in the light thrown upon the Temperature phenomena of the Atlantic Basin by those of the "Cold Area" explored in 1869, it appears clear that we have in the Latitude of Lisbon the same distinct separation between an *upper warm* and a *lower cold* stratum as presented itself in the channel between the Shetland and the Faroe Islands; but whilst the "stratum of intermixture" in the latter lies between 150 and 300 fathoms, it lies in the former between 800 and 1000 fathoms. It seems perfectly clear that the *lower* stratum must have had a Polar source; but there is at present no distinct evidence that the *upper* stratum is derived from any source nearer the Equator. Its temperature, indeed, is *lower* by 4 or 5 degrees than that of the Mediterranean in the same parallel of Latitude at corresponding depths; and since the temperature of the latter may be considered as the *normal* of the Latitude,—this great Inland Sea being virtually excluded from participation in the general Oceanic Circulation,—it would seem that the effect of that circulation is rather to lower than to raise the temperature of the upper stratum of this portion of the Atlantic. Its *surface*-temperature also, as already shown (§ 45), is decidedly lower than that of the Mediterranean under the same parallel; and the limitation of the *superheating* to its most superficial layer is in entire accordance with our Mediterranean observations upon this point (§ 87).—Hence it seems a justifiable conclusion that neither the superficial layer nor any portion of the upper stratum of the Atlantic water that laves the coasts of Spain and Portugal receives any accession of heat from the extension of the Gulf-stream into its area*.

81. When, however, we compare the Temperatures of this upper stratum at different depths with those which are met with at Stations much further North (as tabulated in Par. 110 of the Report for 1869), there is found to be a remarkable correspondence in the *general rate of reduction* with depth, except in this particular,—that the influence of the cold stratum beneath begins to be decidedly marked much nearer the surface; so that instead of a very slight reduction between 500 and 800 fathoms, and then a very rapid passage through a "stratum of intermixture" of 200 fathoms, as in the

* It may be said that this conclusion, though it may be true as regards the *summer* temperature of this marine area, is "not proven" as regards its *winter* temperature. The data furnished, however, by the comparison of the Winter climates of stations along the Atlantic Sea-board, with those of Mediterranean Stations in corresponding Latitudes, indicate that it is as true for Winter as for Summer. (See 'Proceedings of the Royal Geographical Society' for Jan. 9, 1871.)

Southern stations, there is a more gradual reduction of about the same amount (9°) between 500 and 1000 fathoms. Now this result is in remarkable conformity with what might be anticipated on the hypothesis advanced in the last Report. For if the whole *upper* stratum of Oceanic water be slowly moving *northwards* from the region with which we are now concerned towards that explored in the Third Cruise of 1869, whilst the *lower* stratum is slowly moving *southwards* beneath this, it might be expected that the further North the warm stratum advances, the more would it show the influence of the colder stratum beneath, in the lowering of the Temperature of the portion that immediately overlies it. And conversely, in proportion as the cold stratum advances Southwards, we might anticipate that the temperature of *its* upper layer would be gradually raised, so that even when the "stratum of intermixture" has been entirely passed through, we should find the Temperature at corresponding depths somewhat higher than at stations further north,—which is just what seems actually to be the case*.

82. The data at present in our possession seem to point to the inference that the relation between the *upper warm* and the *lower cold* strata of the Ocean, on different parts of the surface of the Globe, is such as may be diagrammatically expressed thus:—



At the Poles (P P, P P) the cold stratum occupies the whole depth, from the surface to the bottom; but as we pass towards the Equator we find it lying further and further down, its surface forming an inclined plane on which the warm stratum rests. The warm stratum, on the other hand, has its maximum depth at the Equator, and gradually thins-off towards the Poles.—This is just what would be expected on the hypothesis of a General Vertical Circulation (§ 124 *et seq.*); since in the Polar-Equatorial flow of the cold stratum its surface would be continually gaining heat by contact with the warm stratum above, so that its superficial portion would be (so to speak) progressively transferred to the warm stratum; whilst, on the other

* Prof. Wyville Thomson, in his Lecture on "Deep-Sea Climates" ('Nature,' July 28, 1870), has expressed the opinion that the cold stratum in the North Atlantic is derived rather from the *Antarctic* than from the Arctic basin. Putting aside the difficulty of accounting for a constant *inflow* of Antarctic water into the Northern Hemisphere, without a corresponding *outflow*, a strong argument against it may be drawn from the facts stated above. For if the *cold* stratum have a Southern source, we should expect its own temperature to be lower, and its effects upon the superincumbent stratum to be more marked, the further *south* it is examined,—the contrary of which proves to be the case.

hand, in the Equatorial-Polar flow of the warm stratum its lower layer would be gradually reduced in temperature by moving over with the cold stratum beneath*. Moreover as its surface would be exposed to a lower and yet lower Atmospheric temperature the further North it moves, each superficial layer as it is cooled will descend into the colder stratum, of which the thickness will be progressively augmented at the expense of that which overlies it.

83. The *Third* group of *Bottom*-temperatures, which includes those taken within the *embouchure* of the Strait of Gibraltar (Nos. 25–28, p. 220), presents some peculiarities which are worthy of notice, when taken in connection with the fact already stated as to the constant Temperature of about 55° found in the water of the Mediterranean at depths greater than 100 fathoms. For if we compare the Bottom-temperatures of Stations 25, 28, 29, 30, and 38, with those of Stations 31, 32, 33, and 34, we find in the former a distinct *elevation* above the latter. Thus at Stations 25 and 28 the temperature was $53^{\circ}\cdot5$, at 374 and 304 fathoms respectively; at Station 29 it was 55° at 227 fathoms; at Station 30 it was $52^{\circ}\cdot7$ at 386 fathoms; and at Station 58 it was 54° at 503 fathoms: whilst at Station 31 it was $50^{\circ}\cdot5$ at 477 fathoms; at Stations 32 and 34 it was 50° at 651 and 414 fathoms respectively; and at Station 33 it was $49^{\circ}\cdot7$ at 554 fathoms. This difference seems fairly attributable to the influence of the undercurrent which is now known to carry out the warmer Mediterranean water to mingle with the colder water of the Atlantic, and which, after flowing over the ridge between Capes Trafalgar and Spartel (§ 69), may still for a time maintain its distinctness on the descending slope of the Atlantic basin. It would probably not be difficult to trace its further course by a sufficient number of observations on the Temperature and Specific Gravity of the bottom-water to the west of the Strait; and it would be very interesting thus to ascertain how far this undercurrent makes its way before blending with the general mass of Atlantic water.—If a detailed examination of the phenomena of the double current should be undertaken by the Authorities at Gibraltar, this point should not be neglected.

84. *Density*.—In order to determine the Salinity of the water of the Atlantic Ocean as a basis for comparison with that of the Mediterranean Sea, the proportion of Chlorine in 34 samples of the former, taken between Falmouth and Lisbon, was determined by Volumetric analysis. Of these samples, 12 were of surface-water, 12 of bottom-water from various depths down to 1095 fathoms, and 12 of intermediate water. The results are expressed in Grammes per 1000 Cubic Centimetres of water.

* It was long ago shown by Dr. Arnott, in his 'Elements of Physics,' that if two layers of water originally of different temperatures, separated by a good conductor, *move in contrary directions*, they will gradually *exchange temperatures*; and this principle is now applied in the construction of Coolers for Breweries and Distilleries, in which a *hot* liquid which it is desired to cool is made to impart nearly all its heat to a *cold* liquid whose temperature it is desired to raise.

	Surface-water.	Intermediate water.	Bottom-water.
Average.....	19·94	19·85	19·75
Maximum....	20·19	19·94	19·98
Minimum....	19·81	19·70	19·46

It appears from these Analyses that there is a slight excess of Salinity in the *surface*-water of the Atlantic, as had been previously observed by Forchhammer*; the excess, however, being so small as not to neutralize the excess of Density which the deeper water derives from its lower Temperature and from the Pressure of the superincumbent column. Five determinations of the Chlorine contained in samples taken at the same spot, from the Surface, and from 10, 25, 50, and 100 Fathoms, gave the following results :—

Surface.....	20·013
10 fathoms	19·909
25 „	19·909
50 „	19·909
100 „	19·805

A comparison of these seems to indicate that the excess of density, being limited to a mere superficial film, is entirely due to evaporation; and the reason why this more concentrated film does not sink, as it does in the Mediterranean (§ 90), is that its excess of Salinity is so small, that even at the depth of 10 fathoms its effect on Specific Gravity is neutralized by the greater density arising from reduction of Temperature.—The difference between the *maximum* and *minimum*, which in Surface-water is only 1·52nd part of the whole, and in Intermediate water only 1·83rd part of the whole, is in Bottom-water 1·38th of the whole; and the *minimum* of the whole series, namely, 19·46, presented itself in a specimen of Bottom-water taken from a depth of 994 fathoms. This proportion might have been presumed to represent the inferior Salinity of the Polar stream, from which the Temperature of the sample indicated its derivation, were it not that another sample from a depth of 1095 fathoms was found to yield 19·73, or nearly the average proportion, of Chlorine. Two samples, taken respectively from 700 and 322 fathoms, gave 19·63; and the maximum of 19·98 occurred in a sample from 717 fathoms. These anomalies are somewhat perplexing, yet the whole range of variation is really very small. Similar anomalies presented themselves in the results of Dr. Frankland's Analyses of samples of Bottom-water collected in the Cold area (Report for 1869, p. 469); for whilst the proportion of Chlorine in a sample (No. 64) taken from a depth of 640 fathoms, at a Temperature of 29°·6, was 19·88, that of the Surface at 49°·7 being 19·96, it was 20·14 in another sample (No. 54), which, though taken at little more than half the depth (363 fathoms), was shown by its Temperature to have been brought up

* See his Memoir on the "Composition of Sea-water" in the Philosophical Transactions for 1865, p. 247.

from the Polar stream, that of the Surface at $52^{\circ}5$ being $20\cdot17$. Possibly, as suggested by Dr. Forchhammer (*loc. cit.*), the several parts of the Polar stream may vary in density, according to the amount of nearly fresh water which each may have derived from the icebergs that have liquefied in it.

85. *Specific Gravity*.—As the determination of Specific Gravity by observations taken with the Hydrometer on board ship is open to two sources of error,—that of the *instrument*, and that of the *reading* (which, when the vessel is unsteady, cannot be precise),—we deem it safer to depend upon the more exact determination of the Specific Gravity of a smaller number of samples by means of the Balance, and to estimate that of others by the Chlorine-determinations. In this manner we arrive at a range of from $1\cdot0261$, the Specific Gravity of the sample of Bottom-water of *minimum* density, to $1\cdot0269$, the Specific Gravity of the sample of Surface-water of *maximum* density; the average of all being $1\cdot0265$. This agrees very well with the results obtained by Forchhammer.

TEMPERATURE AND COMPOSITION OF MEDITERRANEAN WATER.

86. *Surface-Temperature*.—With only two days' exception, the range of the daily average Surface-temperature of the Mediterranean, between the 16th August on which we entered it, and the 28th September on which we quitted it, was between 73° and 79° . The increase at once experienced as we passed into it from Gibraltar Strait was extremely marked (§ 45); and this was maintained for the next two days. On the 19th, however, the average of the day fell from $72^{\circ}2$ to $66^{\circ}9$, the average of the Air being $69^{\circ}8$; and on the 20th it was $68^{\circ}9$, the average of the Air being $74^{\circ}3$. On the first of these days we were crossing from the African towards the Spanish coast, and experienced very strongly the effect of the in-current on the movement of our vessel (§ 47); and it can scarcely be doubted that the low surface-temperature was due to the colder stratum introduced from the Atlantic. On the following day we were between Cape de Gat and Cartagena; and were still within the influence of the cold in-current, which is known to flow past Cape de Gat at the rate of about a mile per hour. On leaving Cartagena, we came into a surface-stratum of true Mediterranean water, as indicated by its temperature of 73° ; and the daily average never afterwards fell below this. The greatest heat was experienced in the neighbourhood of the Tunisian coast, when for several days the average Surface-temperature was 78° . The average Temperature of the Air during the greater part of our Mediterranean Cruise was from 1° to 2° *above* that of the Sea; but during our return from Malta towards Gibraltar, between the 20th and 26th September, the temperature of the Air averaged about $3^{\circ}5$ *below* that of the surface of the Sea, the former having fallen, while the latter remained nearly stationary until we neared the Strait of Gibraltar. As we approached

it, a progressive reduction was observable, from 74°, which had been the average of several previous days, to 72°, with a further reduction to 71° when we entered Gibraltar Harbour. The *scorching* power of the Sun's rays was often very strongly felt; and we much regretted that we were not provided with a Thermometer having a range sufficiently high to enable us to estimate the influence of *direct* solar radiation*. There can be no question that the effect of this radiation upon the surface must be to produce a rapid evaporation, especially when the air is dry. The difference between the Dry- and Wet-bulb thermometers averaged about 4°, but rose occasionally to above 8°; we could not, however, trace any relation between this difference and the Surface-temperature of the Sea.

87. *Temperature of the Upper Stratum*.—Finding that the reduction in Temperature with depth was so extremely rapid as to show that the direct influence of Solar radiation is limited to a comparatively thin stratum of surface-water, we took *Serial* soundings at three Stations, at intervals near enough to show the rate of diminution. The *first* of these Stations (Stat. 40), although the nearest to the Strait, seems to have been out of the direct influence of its cold in-current, which is shown very strongly in the *second* (Stat. 47); the *third* (Stat. 53) may perhaps be taken as representing most characteristically the thermal condition of the upper stratum of the water of the Mediterranean during the season of greatest heat:—

	I.		II.		III.
	° Fahr. Diff.		° Fahr. Diff.		° Fahr. Diff.
Surface	74·5		69·5		77·0
5 fathoms		5·2		10·5	76·0
10 " 	69·3		59·0		71·0
20 " 	65·0	4·3	57·5	1·5	61·5
30 " 	63·0	2·0	56·5	1·0	60·0
40 " 	61·7	1·3	55·7	0·8	57·3
50 " 	59·7	2·0	55·3	0·4	56·7
100 " 	55·1	4·6	54·7	0·6	55·5

Taking No. III., therefore, as the standard of comparison, we observe that while the Thermometer fell only 1° in the *first five* fathoms, it fell 5° in the *second five*, and no less than 9°·5 between 10 and 20 fathoms, below which depth the reduction was very slow. In No. I., with a lower surface-temperature, the reduction in the first ten fathoms was nearly the same; but it was much less between 10 and 20, so that for depths between 20 and 50 it was between 3° and 4° higher than at corresponding depths in No. III.; at 100 fathoms, however, the two were brought to an almost precise accordance by the larger reduction which took place in No. I. between 50 and 100 fathoms. In No. II., on the other hand, the great

* We learned from Colonel Playfair, the Consul General at Algiers, that whilst he was at Aden, a Thermometer with a blackened bulb having been laid upon a black surface, and exposed to the full glare of the Sun, had risen to 215°.

reduction showed itself in the uppermost stratum of 10 fathoms; but though the further reduction took place at a very slow rate, the temperatures at this Station were decidedly below those at the other two, down to 100 fathoms, at which there was not a difference of 1° among the three.

88. *Bottom-Temperature*.—The uniformity which showed itself in the Temperature of the bottom (see Table, p. 221), *at all depths* below 100 fathoms, was very remarkable. The *lowest* bottom-temperature we anywhere met with was 54° ; and this presented itself at a depth of 790 fathoms. The *highest* we anywhere met with was $56^{\circ}5$; and this presented itself in three instances, at depths of 266, 390, and 445 fathoms. But that the trifling elevation was not in any way dependent upon the smaller depth of these Soundings, was obvious from two considerations:—first, that our deepest sounding gave a temperature of 56° on a bottom of 1743 fathoms, whilst we found 55° at depths of 1456 and 1508 fathoms; and second, that the slight variations observable among the *bottom*-temperatures occurred also between the temperatures taken at 100 fathoms. In fact, *whatever the temperature was at 100 fathoms, that was the temperature of the whole mass of water beneath, down to the greatest depth explored*. In that part of the Western basin of the Mediterranean which lies between Gibraltar and Sardinia, the bottom-temperature ranged between 54° and $55^{\circ}5$, the *average* being $54^{\circ}9$. East of this, in the neighbourhood of Sicily, the bottom-temperature ranged between 55° and $56^{\circ}5$, the *average* being $55^{\circ}8$. It was because we thought it possible that the slight excess of Bottom-temperature on this area might be due to Volcanic heat beneath, that we directed our homeward course by way of Etna and Stromboli, for the purpose of ascertaining if the near neighbourhood of a constantly active Volcano has any influence in raising the temperature of the bottom. No such influence, however, was perceptible; the Temperatures obtained at Stations 61 and 62,—namely $55^{\circ}7$ at 392 fathoms, and $55^{\circ}3$ at 730 fathoms,—being rather *below* than above the average.

89. The remarkable contrast thus presented to the slow but continuous reduction of temperature encountered in the successive strata of Oceanic water in the great Atlantic basin, and to the sudden fall which presents itself as the Thermometer descends to its lower depths (§ 79), excites enquiry into the cause of the difference. It is now clear that no amount of *surface*-heat has power *directly* to affect the temperature of sea-water to a greater depth than 100 fathoms, the elevation of temperature it produces below 30 fathoms being very slight; and it seems also clear that the uniform temperature of from 54° to $56^{\circ}5$ encountered below the 100 fathoms' stratum, represents the *permanent temperature* of the great mass of water which occupies the Mediterranean basin. Now this mass is entirely cut off from the influe of the General Oceanic Circulation, the surface-inflow through the Strait of Gibraltar having no other effect than slightly to lower the general temperature at the western extremity of the basin. And the uniform permanent temperature of the mass of Mediterranean water may

thus be considered as representing the mean temperature of the Earth in that region, slightly raised, perhaps, by a *downward convection* of heat from the surface in the manner to be presently described (§ 90). With such an allowance it corresponds closely with the determinations of the mean temperature of the Crust of the Earth, made by sinking Thermometers into the ground to such a depth as to seclude them from the direct influence of Summer heat or Winter cold, but not to bring them within the direct influence of the Internal Heat of the earth. Thus Quetelet found that a Thermometer sunk to a depth of 24 feet at Brussels showed an annual average of $53^{\circ}4$, the range of variation being only $2^{\circ}5$; and Bischoff found a Thermometer sunk to a depth of 36 feet at Bonn give an annual average of 51° , with a range of only $1^{\circ}5$. The Temperature of deep Caves gives another set of data of the like kind, which accord very closely with the foregoing. Thus we have been informed by Mr. Pengelly that the temperature in the part of Kent's Hole at Torquay which is furthest from its entrance varies but little from 52° throughout the year. There is a cave in the island of Pantellaria, lying between Sicily and the African Coast, which is reputed to be of "icy coldness;" but Lieut. Millard, of H.M.S. 'Newport,' who has lately been making a careful survey of the Island, informed us that, although he felt it "very cold" on passing into it out of a very hot sunshine, its actual temperature, taken by Thermometer, was 54° . And we have also learned on good authority that this is the temperature of the bottom of the deepest tanks in which water is stored up in Malta, provided that these are excavated (as is very commonly the case) beneath the houses, or are in any other way secluded from the direct rays of the sun.

90. Now let it be supposed that the superficial stratum of the water of the Mediterranean had been cooled down by a severe winter to the uniform temperature of the depths below; we have to enquire in what manner it would be affected by the heating-power of the summer sun. This, it is obvious, can be only exerted *directly* upon the actual surface; for the *conducting*-power of water is so small that very little downward transmission of heat would take place through its agency. Further, as the application of heat at the surface will render the superficial layer specifically lighter, no such *convection* will take place in the *downward* direction as takes place *upwards* when heat is applied at the bottom. But another agency comes into play in the case of Sea-water. The rapid evaporation produced by powerful solar radiation, especially when aided by the hot dry winds of Africa, occasions such a concentration of the surface-film, that, in spite of its elevation of temperature, it becomes specifically heavier, and descends,—to be replaced by a fresh layer. In this manner it will carry down an excess of heat, which diffuses itself through the subjacent layer, of course producing the greatest elevation of temperature in the stratum nearest the surface. The continual repetition of this process through the hot season will carry the elevation of temperature further and further

down ; but so soon as the temperature of the Air falls much *below* that of the Sea, the surface-layer being cooled will become heavier and sink, and will thus carry down *cold* instead of heat, so as to lower the temperature of the stratum below. In no instance, however, so far as we can learn, has the surface-temperature of the Mediterranean ever been seen so low as 56° , even in midwinter.

91. That it is by this sinking of the surface-films successively concentrated by evaporation that the Solar heat, which acts so powerfully on the Mediterranean basin during the summer, is transmitted downwards, appears certain from the fact, of which the particulars will be presently given, that the Salinity of the water of the Mediterranean is greater *below* the surface than *at* the surface, instead of diminishing as it does in the Atlantic (§ 84) ; and we thus see how important an influence is exerted by that Salinity in diffusing the heat imparted to the surface through the waters' beneath. In the great *fresh*-water lakes of Switzerland, the deeper water retains all through the year a temperature but little above 39° , the small excess being probably derived from the warmth of its bed ; for the whole mass of water down to the bottom must be cooled to this degree in winter before any ice can form on its surface ; and as the heating of the surface in summer makes all the water affected by it specifically lighter, none of it will descend and carry heat downwards, as it does in the Mediterranean.

92. *Density*.—The determination of the actual Salinity of the water of the Mediterranean basin, alike at the Surface and at various Bottom-depths, was one of the special objects of our inquiries ; for although various Analyses had been previously recorded, they had been made upon samples of water which had been kept in bottles for a more or less considerable period ; and the depths from which those samples had been collected were not by any means the greatest known to exist in this basin.—The number of Chlorine-determinations of *Surface*-water was 25 ; and their Geographical range was from the Strait of Gibraltar to the edge of the Eastern basin (Station 60). A marked difference in density was observable between the *Surface*-waters of the Western and of the Eastern portions of this area ; for whilst those of the latter invariably showed a considerable excess in Salinity above the *maximum* of Atlantic water, that excess was so much reduced in some of the samples taken nearer to the Strait, as almost certainly to show that the surface-stratum there consists in great degree of Atlantic Water. Thus at Station 47, in which a like indication was given by the Temperature of the *Surface*-water (§ 87), we found the proportion of Chlorine to be 20·46, or only 0·27 above the maximum we had encountered in Atlantic water ; and when we crossed to the neighbourhood of the opposite Algerine Coast (where, however, the density of the *Surface*-water was probably reduced by the entrance of *River*-water), we found the proportion of Chlorine as low in one case as 19·69, and in another as 19·99. When approaching the Strait on our return voyage, we took a series of five samples

for the purpose of testing the reality of this difference ; and we found the proportions of Chlorine to be respectively 20·77, 20·67, 20·56, 20·51, and 20·47. The *mean* of these five determinations, together with the one previously taken at Station 47, but excluding the two taken on the coast of Algiers, is 20·57 ; and the mean Specific Gravity was 1·0274. On the other hand, at the Sicilian end of the basin, where the water was that of the Mediterranean proper, the *mean* of ten Chlorine-determinations was 21·05, with a corresponding Specific Gravity of 1·0280. The *maximum* of Chlorine was there 21·32, with Specific Gravity 1·0284 ; and the *minimum* 20·77, with Specific Gravity 1·0277. The combination of these 16 observations gives a *mean* of 20·87 for the Chlorine, and 1·0278 for the Specific Gravity, of the Surface-water of the Mediterranean generally *.

93. The number of Chlorine-determinations of *Bottom-water* taken in the proper Mediterranean basin, at depths between 207 and 1700 fathoms, was 20. They show a general excess of Salinity over the Surface-water, the *mean* of the whole being 21·38 (as against 20·87), with a *maximum* of 21·88 (Sp. Gr. 1·0292) and a *minimum* of 21·08 (Sp. Gr. 1·0281). On grouping them into three Series according to their depth, we arrive at a curious result :—

Fathoms.		Chlorine.	Sp. Gr.
200 to 400,	Mean of 7 observations.	21·53	1·0287
400 to 800,	„ 7 „	21·38	1·0285
1300 to 1700,	„ 6 „	21·21	1·0283

94. Thus it appears that the excess of Salinity is greatest in the *shallower* water, and that it gradually diminishes with the depth. This is also shown most strikingly by comparing the sample taken from the *least* depth (207 fathoms) with that taken from the greatest depth (1703 fathoms) ; for it was the former that showed the *maximum* of 21·88, and the latter that showed the *minimum* of 21·08.—Now this fact, though at first unexpected (since we had supposed that the heaviest water would gravitate to the greatest depths), seems not difficult to account for, if we consider the mode in which the concentration of the surface-film will be likely to affect the water below. For it can be shown experimentally, by pouring a strong saline solution tinged with colour upon the top of a weaker colourless solution, that the former will in the first instance sink “bodily,” but will gradually impart its excess of salt to the liquid through which it falls ; the descent of the coloured stratum becoming slower and slower, and its

* This *mean* accords closely with that of 20·845 obtained by Prof. Forchhammer from his examination of samples collected at different times and by different persons from various parts of the surface of the Mediterranean (Phil. Trans. 1865, p. 252). His *maximum* of Chlorine, 21·718, was higher than ours ; but this seems to have been an exceptional case ; and the sample may have undergone some concentration in keeping. On the other hand, his *minimum* was lower, being only 20·16 ; but this sample, having been taken in the Strait of Gibraltar, contained a large proportion of Atlantic water.

colour being more and more imparted to the general mass of the liquid. The proportion of salt will in time be made uniform throughout the whole column by "diffusion." Now it is obvious that if each column rests (so to speak) on its own base, the degree in which the Salinity of the whole mass is raised by the addition of a more concentrated solution will depend *cæteris paribus* upon its height; and thus where the depth of the Mediterranean basin is only between 200 and 400 fathoms, we should expect the Specific Gravity of its water to be more raised by the successive concentration of its surface-films, than where the depth ranges from 1300 to 1700 fathoms.

95. Since this proves actually to be the case, the further conclusion appears justifiable—that there is *an extremely small amount of movement in the abyssal waters of the Mediterranean basin*. The uniformity of Temperature throughout the whole of it, and the restriction of seasonal changes in temperature to its upper stratum, will prevent it from being subjected to any thing like the *vertical circulation* which is produced in the great Oceanic basins by the antagonistic action of Heat and Cold on the Equatorial and Polar areas (§ 125). And from any *horizontal displacement* they would seem altogether excluded by the depth at which they lie; for the action of winds cannot disturb more than that comparatively superficial stratum which is affected by the Gibraltar current. The inflow of lighter surface-water through the Strait, and the outflow of denser water from the comparatively shallow stratum of the neighbourhood, will probably produce no change whatever at depths greater than 500 fathoms. And the same may be said of the supply of fresh water brought in either by rain or rivers; for this will at once go to make up the loss produced by surface-evaporation; and whilst helping to maintain the purity of the upper stratum inhabited by fishes &c., will do nothing for the waters of the abyssal depths. If these waters were continually subject to horizontal displacement, it might be expected either that the heaviest water would gravitate to the greatest depths, or that the density of the entire contents of the deeper portion of the basin would be equalized; neither of which happens. On the contrary, as just shown, the density varies with the depth in so marked a degree, as to indicate that the water in each part of the basin retains its distinctness from the rest through long periods of time.

96. *Solid Matter in Suspension*.—The water of the Mediterranean is distinguished from that of the Atlantic, not only in the larger proportion of Saline matter which it holds in *solution*, but also in having diffused through its whole mass, in a state of *suspension*, particles of solid matter in an extremely fine state of division. This statement may seem strange to those who are familiar, either by personal observation, pictorial representation, or verbal description, with the (apparently) clear deep blue of the Mediterranean Sea. But the two phenomena will be presently shown not only to be compatible, but to stand to each other in the relation of cause and effect.

97. Our attention was drawn to this point, in the first instance, by finding that the bottom-water brought up by the Water-bottle was nearly always turbid, and that this turbidity was with difficulty removed by filtration. The bottom-water brought up from sandy or gravelly bottoms is always clear; and though that which was brought up from the area covered by the "Atlantic mud" was often turbid, it was readily cleared by passing through filtering-paper, the deposit on which was found to consist of very minute *Globigerinæ*, which had been apparently floating in the stratum immediately above the Sea-bed. As the clearing of the Mediterranean water was requisite for our Chlorine-determinations, it was passed twice or thrice through the filter, and the solid matter left upon the paper consisted entirely of Inorganic particles of extreme minuteness. Now it is a fact well known to Chemists and Physicists, that the length of time required for the deposit of a precipitate increases with the fineness of the division of its particles, notwithstanding that the material of which they are composed may be of very high Specific Gravity. Thus it was shown by Faraday that precipitates of Gold may not subside for a month; and Mr. Babbage has calculated that, in the case of lighter substances, a period of hundreds of years may be required for the gravitation of very finely divided particles through a considerable mass of fluid.

98. Taking into account, therefore, that the deep waters of the Mediterranean are not only cut off from the General Oceanic Circulation, but that they are almost entirely destitute of *vertical* circulation amongst themselves (§ 95), it may be fairly considered that the perceptible turbidity of the bottom-water is due to the imperceptible diffusion of the same finely divided matter throughout the entire mass of superincumbent water. And that this is really the case, is shown by two different methods of proof. We learned from the Engineer of the Peninsular and Oriental Company's Steam-ship by which we proceeded to join the 'Porcupine' at Gibraltar, that the deposit removed from the boilers after working in the Mediterranean differs from that left by Atlantic water, not only in its larger proportion of *salt*, but in having a very finely divided *mud* diffused through it, which is, of course, derived from the evaporation of *surface-water*. The result of this large-scale experiment harmonizes exactly with that of Prof. Tyndall's examination of a small sample of the surface-water of the Mediterranean by the Electric light; for he found it to be highly charged with minute particles in suspension, as is also the water of the Lake of Geneva. And he has further shown that it is in each case to the presence of these particles that we are to attribute the peculiar intensity of the blue colour by which both these waters are characterized*.

* See 'Nature,' Oct. 18, 1870.—We may take leave to mention that the same idea of the agency of the suspended particles in intensifying the blue colour of the water had previously occurred to ourselves, and had been made the subject of conversation on our voyage home, the probable community of the source of the suspended particles in the Mediterranean and the Lake of Geneva respectively having especially presented

99. But further, when we come to enquire into the source of these suspended particles, the progressive subsidence of which gives rise to the fine muddy deposit that covers all the deeper parts of the Mediterranean, we find that (so far, at least, as the Western basin is concerned) they have been in all probability brought down into it by the Rhone. The upper part of that river, as is well known, is constantly transporting a vast mass of sedimentary matter into the Lake of Geneva; and while the deposit of the coarser particles of the sediment at the upper end of the Lake is causing a progressive formation of alluvial land, the water which passes off at the lower end, though apparently clear, is still charged with particles in a finer state of division. "Scarcely," says Sir C. Lyell*, "has the river passed out of the Lake of Geneva, before its pure waters are again filled with sand and sediment by the impetuous Arve, descending from the highest Alps, and bearing along in its current the granitic sand and impalpable mud annually brought down by the glaciers of Mont Blanc. The Rhone afterwards receives vast contributions of transported matter from the Alps of Dauphiny and the primary and volcanic mountains of Central France; and when at length it enters the Mediterranean, it discolours the blue water of that sea with a whitish sediment for the distance of between six and seven miles, throughout which space the current of fresh water is perceptible."—Thus the Western basin of the Mediterranean stands in the same relation to the lower part of the Rhone and to the tributaries which discharge themselves into it, that the Lake of Geneva does to its upper part. And a like universal diffusion of fine sedimentary particles through the Eastern basin is probably effected by the transporting agency of the Nile.

100. The very slow, but constant, subsidence of these minute sedimentary particles, then, is the source of a large part of the material of that fine tenacious Mud which, mingled with a larger or smaller proportion of Sand, partly calcareous and partly siliceous, constitutes the deposit at present in progress on the deeper parts of the Mediterranean sea-bed. The source of the Calcareous sand, which is itself in a state of very minute subdivision, is probably to be found in the abrasion of the Calcareous Tertiaries which form the shore-line round a large part of the Western basin. This abrasion is specially noticeable at Malta, where, for the security of the fortifications, it has been found necessary to check it by artificial means. The singular barrenness of this deposit in regard to Animal life forced itself upon our attention during the whole of our dredging-operations in the Mediterranean (§§ 48–52); and whilst disappointed as Zoologists in not meeting with the novelties we hoped to encounter, we venture to hope that

itself to us: and it was with great satisfaction, therefore, that we found our notion so fully confirmed by Prof. Tyndall's investigations; whilst it was not a little interesting to him to find that our independent inquiries had led us to affirm the presence of these suspended particles through the whole mass of Mediterranean water, and to attach so much importance to the fact in its Geological and Biological relations.

* *Principles of Geology*, 10th ed. vol. i. p. 427.

the negative result of our sedulous investigations may have an important Geological bearing.

101. It will be borne in mind that our previous researches have fully demonstrated the fact that the Depth of from 600 to 1200 fathoms is not *per se* inconsistent with the existence of a varied and abundant Fauna; and that the reduction which shows itself at from 1200 to 2435 fathoms seems to depend as much on depression of Temperature, as on increase of Depth. Hence it was fairly to be expected that a varied and abundant Fauna—probably containing a number of Tertiary types supposed to have been long extinct—would have been found between 500 and 1500 fathoms, on a bottom of which the temperature seems never to fall below 54°. Now the question as to the cause of the deficiency of Animal Life on this bottom naturally connects itself with the old Geological difficulty, of which the inquiries of Prof. Edward Forbes were long supposed to afford a satisfactory solution; viz. the existence of vast thicknesses of sedimentary strata, almost or entirely destitute of Organic Remains. The explanation which has been accepted for many years,—that these deposits were formed in Seas too deep to allow of the existence of Animals on their bottom,—having been now shown to be untenable, the old difficulty recurs; and it is obvious that if it can be shown that a condition prejudicial to Animal Life now prevails on the Mediterranean bottom, which also prevailed when other azoic deposits were formed, a great step will have been gained. Such a condition is to be found—we are disposed to think—in the *turbidity of the bottom-water*. All Marine animals are dependent for the aeration of their fluids on the contact of water either with their external surface, or with special (branchial) prolongations of it. Now if this water be charged with suspended particles of extreme fineness, the deposit of these particles upon the respiratory surface will interfere with the aerating process, and will tend to produce asphyxia. This is not a mere hypothesis. It is well known that Oyster-beds cannot be established in situations to which fine mud is brought by any fluvial or tidal current. And our Colleague Mr. Jeffreys, when dredging some years ago in the neighbourhood of Spezzia, having on one occasion passed a little out of the Bay, from a sandy bottom rich in Animal life to a muddy bottom (this mud being doubtless a part of the Rhone deposit), without any considerable increase of depth, was forcibly struck by the barrenness of the latter.

102. It will be for Geologists to say how far this explanation can be applied to the case of the azoic sedimentary deposits of former epochs. One very notable case of the kind has been communicated to us by Dr. Duncan, that of the Fleisch, a stratum not less than 6000 feet thick, extending from Mont Blanc to the Styrian Alps, which must have been deposited in the condition of extremely fine arenaceous mud, and in which there is an almost entire absence of Fossils. We are disposed to believe, also, from the results of such inquiries as we have been able to make, that the extremely fine Calcareous sandstone of Malta, though reputed to be

rich in Fossils, will be found to contain these fossils, for the most part, in its coarser beds; which were probably deposited in shallower waters, like those which we found rich in Animal life along the shores of the Mediterranean. The extremely fine stone that is used for carved work,—so entirely wanting in “grain” that carvings executed in it look like casts in Plaster of Paris,—contains, we were assured, few fossils except Sharks’ teeth, which, of course, dropped into it from above. We commend this enquiry to the attention of Maltese Geologists, as one having an important bearing on the solution of a problem of the highest interest.

103. There is another condition, however, which may be not less potent in restraining within very narrow limits the Animal Life of the deeper parts of the Mediterranean Basin,—namely, *the stagnation produced by the almost entire absence of Vertical Circulation*. In the great Oceanic basins, if our doctrine (§ 124 *et seq.*) be correct, every drop of water is in its turn brought to its surface and exposed to the purifying influence of prolonged exposure to the air. From this movement, the water of the Mediterranean may be said to be virtually excluded; and, as has been already shown (§ 95), the deeper part of the basin has no Circulation of its own, either horizontal or vertical, which will have the effect of bringing its water to the surface. It is difficult, in fact, to conceive of *any* agency that can disturb the stillness of the abyssal depths of a basin which is completely shut in by a wall that rises more than 10,000 feet from its bottom. How far this affects the condition of such depths, in respect to the diffusion of the Organic matter and the Oxygen required for the support of Animal life, must be a matter of future inquiry.

GIBRALTAR CURRENT.

104. The term “Strait of Gibraltar” is usually applied to that space between the coast-lines of Spain and Morocco which is bounded on the west by Capes Trafalgar and Spartel, and on the east by the two “Pillars of Hercules,”—namely Europa Point, which forms the southern extremity of the Rock of Gibraltar, and Jebel Musa or Apes Hill on the Barbary side. As Admiral Smyth justly remarks*, however, we may correctly include in the Strait the whole of that funnel-shaped entrance from the Atlantic, of which the western boundary is formed by a line from Cape St. Vincent on the north to Cape Cantin on the south, the whole of the water within this entrance being affected by the surface-draught into the Mediterranean. It was considered by Major Rennell that there is a general “set” of Atlantic water between Lat. 30° and 45° N., and from 100 to 130 leagues off the land, towards the entrance of the Strait, the rate of movement being as much as from 14 to 17 miles per day. This estimate, however, is regarded by Admiral Smyth (*loc. cit.*) as excessive; although he thinks that such an indraught may possibly occur during the long pre-

* The Mediterranean, p. 158.

valence of particular winds. We have been informed by Admiral Ommaney that, according to his own experience, the *inset* is most considerable towards the African coast, while the *outset* occasionally observed (§ 106) is most decided on the Northern coast ; and this statement derives very remarkable confirmation from the Thermometric observations already detailed. For these show that the lower temperature of the *in*-current is specially noticeable near Cape Spartel (§ 73) ; whilst the higher temperature, which is traceable westwards along the Spanish and Portuguese coasts as far as Cape St. Vincent, and there suddenly falls to the ordinary standard of the Atlantic, seems to be derived from an *efflux* of the Mediterranean water (§ 75).

105. The length of the narrower portion of the Strait (see Chart II.), sometimes distinguished as the Gut, is about 35 miles. Its width, which is about 22 miles between Capes Trafalgar and Spartel, gradually diminishes to somewhat more than 9 miles between Tarifa and Alcazar point ; and then increases until it reaches 12 miles between Gibraltar and Ceuta, eastward of which the Strait terminates abruptly in the wide basin of the Mediterranean. The deepest portion of the Strait is at its eastern extremity ; its depth between Gibraltar and Ceuta reaching 517 fathoms, and averaging about 300 (Section c d.). From this the bottom gradually but irregularly slopes upwards (Section a b) as far as the western extremity of the Gut, where the shallowest water is to be found. The northern half of the channel across the section between Capes Spartel and Trafalgar (Section x r) scarcely anywhere exceeds 50 fathoms ; whilst its southern half does not seem anywhere to reach 200, and may be considered to average 150 fathoms. On the Atlantic side of this ridge the bottom gradually slopes downwards, until it reaches, at 40 miles westward, a depth about equal to that which it has between Gibraltar and Ceuta. This ridge, therefore, constitutes a kind of marine "watershed," separating the Inland basin of the Mediterranean from the great Oceanic basin of the Atlantic.

106. Through the central part of this Strait a current almost invariably sets eastward, or from the Atlantic into the Mediterranean. This current is most rapid in the narrower part of the Gut, where the *inflow* usually has a rate of from *two* to *three* miles an hour ; this rate sometimes rising to *four* miles, or even occasionally (as stated by Gibraltar pilots to Admiral Smyth) to *five* ; whilst the current is sometimes so reduced in speed as to be scarcely perceptible, even giving place (though very rarely) to a contrary movement or *outflow* from the Mediterranean towards the Atlantic. These variations are partly due to Tidal influence, which is here very decided, and which may either concur with or oppose the general current. When the tide is *flowing*, its motion is *westwards*, or in opposition to the current ; and at spring-tide this motion may be sufficiently powerful to check the current, or even to reverse its direction for a short time. When the tide is *ebbing*, on the other hand, its motion is *eastwards*, or in the direction of the current ; and it is to the ebb of spring-tides that the occasional augmentation in the rate of the current to 5 miles an hour is probably

attributable. These effects will, of course, be most marked when the tidal movement is augmented by a strong wind in its own direction*.—The constant current does not occupy by any means the entire breadth of the Strait. In its narrowest part the rapid central in-current is said by Admiral Smyth not to average more than 4 miles in width; and on either side the stream when moving inwards is usually much less rapid, its rate, independent of tide, being about 1 mile per hour in the neighbourhood of Tarifa, and 2 miles an hour on the coast of Africa. These lateral currents are much more affected than the central current by the Lunar tide, which produces a complete periodical reversal of them; the flood at springs running *westwards* off Tarifa at the rate of from 2 to 3 miles per hour, whilst even at neaps it runs westwards at 1 mile per hour. The ebb, on the other hand, concurs with the general current, and augments both its volume and its rate. Thus, when the water is falling, the whole stream is running eastwards; but when it is rising, the tide on either shore sets westwards. By taking advantage of this periodical westerly flow in the lateral streams, it is possible for sailing-vessels to make their way outwards† in opposition to a continued westerly wind.

107. The rate of the general in-current diminishes immediately that it discharges itself into the Mediterranean basin, over the surface of which it seems to spread itself, in virtue of its lower Specific Gravity (§ 92). But the influence of its motion is sensibly experienced along the Spanish Coast as far as Cape de Gat, and along the African Coast even as far as the Bay of Tunis,—its force and direction, however, being greatly affected by the prevalent winds.

108. Various hypotheses have been put forward at different times to account for this continual influx of Atlantic water into the Mediterranean. The motion of an undercurrent flowing in the opposite direction was very early suggested by Dr. Smith‡; but he did not attempt to show in what way motion is given either to the surface inflow or to the deep outflow. Quite recently the extraordinary hypothesis has been seriously put forward, that the influx through the Strait may be due to a gradual depression of the bottom *now going on*§. The explanation usually received is that first offered by Dr. Halley||, who attributed it to the excess of evaporation from the surface of the Mediterranean Sea over the whole amount returned to its basin either directly by rainfall or by the rivers which discharge themselves into it; so that the level would be progressively lowered, if not kept up by an inflow from the Atlantic. The obvious objection to this explanation is, that as the water which passes off by evaporation leaves its Salt behind it, and as the water which enters through the Strait is

* Admiralty Sailing Directions, p. 309.

† See Admiral Smyth's 'Mediterranean,' p. 176.

‡ Philosophical Transactions, vol. xiv. p. 364.

§ Mr. George Maw in Geological Magazine, December 1870, p. 550.

|| Philosophical Transactions, vol. xvi. p. 366.

charged with the ordinary proportion of salt, there must be a progressive increase in the density of the water of the Mediterranean until it reaches the point of saturation. This objection has been met by another hypothesis, viz. that although the surface-water of the Mediterranean shows very little excess of density, there may be a great increase in the proportion of salt held in solution in the waters of its abyssal depths; and it has even been surmised that a deposit of salt is taking place on its bottom.

109. This hypothesis seemed to derive support from the analysis made by Dr. Wollaston in 1828*, of a sample of bottom-water brought up by Admiral Smyth from a depth of 670 fathoms, at a point about 50 miles within the Strait; which analysis gave the extraordinary percentage of 17·3 parts of Salt, with a Sp. Gr. of 1·1288,—the proportion of Salt in ordinary sea-water being about 3·5 per cent., and its usual Sp. Gr. about 1·027. But as Dr. Wollaston's analyses of two other samples of Mediterranean water, taken respectively from depths of 450 and 400 fathoms, at distances of 680 and 450 miles eastward of the Strait, showed that their density but little exceeded that of ordinary sea-water, it was pretty clear that the first result was anomalous, and that in whatever way it was to be accounted for†, it did not represent the general condition of the deep water of the Mediterranean. (See §§ 43, 44.)

110. The inquiries detailed in the previous Section of this Report have conclusively shown (1) That there is a general excess of Salinity in the water of the Mediterranean over that of the Atlantic; (2) That this excess does not pass beyond very narrow limits; (3) That it is *least* in surface-water the proportion of salt in which is only about 4·7 per cent. above that contained in the surface-water of the Atlantic; (4) That it is *greatest* in bottom-water the proportion of salt in which may reach about 9 per cent. above that contained in the bottom-water of the Atlantic, this last *not* being *more*—and apparently somewhat *less*—dense than the surface-water of the same ocean. Our inquiries were almost entirely limited to the Western basin, in which bottom-water of the highest density seemed to prevail at the shallower depths (§ 93). The single sounding which was taken in the Eastern basin, at a depth greater than any elsewhere reached, gave us a sample of which the excess was 6·7 per cent.

111. From these results it seems a justifiable inference that the evaporation from the water of the Mediterranean basin is in excess of the amount of fresh water returned into it, occasioning an increase of its density; but that this increase, notwithstanding the constant influx of salt water from the Atlantic, is in some way kept in check, probably through an efflux of the denser water by an undercurrent, as originally suggested by Dr. Smith in 1673. This is the view adopted by Sir John Herschel (*Physical Geography*, 1861, p. 28); and it has been considered to derive support from

* Philosophical Transactions, 1820, p. 29.

† It was suggested by Admiral Smyth ('Mediterranean,' p. 131) that a brine spring might have been struck upon.

accounts that have been recorded of vessels sunk in the narrower part of the Strait having floated up near Tangier*. But to these accounts no great importance is assigned by Admiral Smyth†, who seems inclined to attribute the occurrences—if they really took place as narrated—to the action of the lateral Surface-outflow.

112. The only objection that has been advanced, so far as we are aware, to the hypothesis of a westerly undercurrent, is based on the existence of the comparatively shallow ridge which (as already stated) crosses the western end of the Gut between Capes Trafalgar and Spartel. The existence of this ridge, in the opinion of Sir Charles Lyell‡, “has dispelled the idea which was once popular that there was a counter-current at a considerable depth in the Straits of Gibraltar, by which the water which flows in from the Atlantic is restored to that ocean.”—But the validity of this objection has been disputed, and we think successfully, by Captain Maury, who, after citing many cases in which a deep current comes up to near the surface, concludes as follows:—“To my mind the proofs derived from reason and analogy are as clear in favour of this undercurrent from the Mediterranean, as they were in favour of Leverrier’s planet before it was seen through the telescope at Berlin”§.

113. The analogy of the Red Sea and the incurrent through the Strait of Babelmandeb, which is adduced by Capt. Maury in support of this view, is a very cogent one. The evaporation from the Red Sea is well known to be enormous, its annual amount being estimated by Dr. Buist as equal to a sheet of water eight feet thick, corresponding in area with the whole expanse of that sea. Of the whole amount of *fresh* water thus drawn off, scarcely any is returned either by rivers or rains. But the level is kept up by a strong current that continually sets in through the Strait of Babelmandeb; and as this current brings in *salt* water, there would be a continual and very rapid accumulation of salt in the trough of the Red Sea if the denser water were not carried off by an outward current beneath. Now since all the observations hitherto made upon the density of the water of the Red Sea show it to be very little greater than that of the Indian Ocean||, there would seem no escape from the conclusion that such a reverse undercurrent must really exist.

114. We shall now present, in a concise and connected form, the General Results of the inquiries we have ourselves made to determine this question; the particulars having been detailed, as they presented themselves, in the preceding Narrative. These results were of a twofold character. It was our object, (1) to detect if possible by *mechanical* means any *movement* which may be taking place in the lower stratum of water in opposition to

* Philosophical Transactions, vol. xxxiii. p. 102.

† Mediterranean, pp. 154–157.

‡ Principles of Geology, 10th edit. vol. i. p. 563.

§ Physical Geography of the Sea, 1860, pp. 194–196.

|| Transactions of Bombay Geographical Society, vol. ix. p. 39.

the surface inflow ; and (2) to determine, by the *Temperature*, the *Specific Gravity*, and the *Composition* of samples of water taken up at different points and from different depths, whether they had been drawn from the Atlantic or from the Mediterranean basin.—The *mechanical* method was entirely devised and carried out, with the practical ability for which he is eminently distinguished, by our excellent friend Staff-Captain Calver (see § 37). The *physical* and *chemical* observations, which were made under our own direction, gave results which harmonize completely with those of the mechanical, where both could be employed together ; and supply a deficiency which the impossibility of applying the mechanical test on the uneven bottom of the shallow ridge would otherwise have left, in the proof of the outflow of Mediterranean water over it.

115. Our investigations were first made in the mid-stream between Gibraltar and Ceuta, at nearly the narrowest part of the Strait, where its depth exceeds 500 fathoms (Chart II. Section c d). The decided retardation of the boat by the “current-drag” at 100 fathoms in both sets of experiments (§§ 40, 62) showed that the in-current at that depth has *less than half* the velocity of the surface-current. When the “current-drag” was lowered to 250 fathoms, there was in the First set of experiments simply a further increase of retardation, the boat being kept almost in a stationary position : we felt justified, however, in inferring that the strain of the “current-drag” could not have so nearly neutralized the action not only of the surface-current, but also of the wind, upon the boat from which it was suspended, if it had not been itself acted on by a counter-current. And this view derived very strong confirmation from the evidence afforded by the *Temperature*, the *Specific Gravity*, and the *Density* of the water in the 250 fathoms’ stratum. For, in the first place, the surface-temperature being 66°, and the temperature at 100 fathoms having fallen to 55°·7, no further reduction showed itself below that stratum ; the water at 250 fathoms, like the bottom-water at 517 fathoms, having exactly the same temperature as the water at 100 fathoms. This, as we have seen, is the uniform rule in the Mediterranean, whilst far otherwise in the Atlantic. Further, the *Specific Gravity* and the proportion of Salt in the water at 250 fathoms indicated a density which no Atlantic water possesses, and which was not exceeded in any sample obtained from the Mediterranean. There could be no question, therefore, that the stratum at 250 fathoms must be Mediterranean water ; so that, if not absolutely stationary, it must be moving westwards. Now this westerly movement was distinctly demonstrated in our Second set of experiments, by the motion of the boat from which the “current-drag” was suspended (§ 62) ; and since the observations on the *Temperature*, *Specific Gravity*, and *Salinity* of the water in this stratum, which were then repeated, gave results almost precisely identical with those made on the previous occasion, it seems fair to conclude that there was a westerly current in this stratum in the First, as well as in the Second instance, though its effect on the current-drag was masked by the stronger antago-

nistic forces acting on the boat.—The same observations apply to the 400 fathoms' stratum. In the First set of experiments, the boat moved *with* the surface-current, but at little more than *a quarter* its rate; and this retardation, taken in connection with the distinct Physical and Chemical indications that the 400 fathoms' stratum was Mediterranean and not Atlantic water, might fairly be taken as evidence that the force acting on the "current-drag" was antagonistic in its direction to the surface-forces acting on the boat, though less powerful than in the 250 fathoms' stratum. This inference also was justified by the results of the Second set of experiments (§ 62), which showed us the boat carried *westwards*, though at a less rate than when the "drag" hung in the 250 fathoms' stratum.—It was not a little remarkable to find in both sets of observations, that the water of this *lower* stratum is of *less* density than that which overlies it at 250 fathoms, though still unmistakably Mediterranean; and it may hence be pretty certainly inferred that the denser *middle* stratum is drawn by current-action from some intermediate part of the Mediterranean basin at which the maximum density prevails (§ 93), and that it is flowing with a gradual upward inclination, so as at last to pass over the ridge at the opposite extremity of the Strait. On no other hypothesis does it seem possible to explain the persistence of this condition,—supposing it to be uniform, as the close conformity of observations made after an interval of six weeks would indicate that it is.

116. Although we should have been very glad to repeat our experiments at some intermediate Section, yet, as our time did not allow of our carrying them out in more than one other locality, we considered it desirable to proceed at once to the western extremity of the Strait, where its breadth greatly increases, whilst its depth is yet more than proportionally reduced. As already stated (§ 66), the bottom is here characterized by great inequalities; channels of from 150 to 190 fathoms' depth existing in the immediate neighbourhood of shallows of not less than 50 fathoms (see Section *EF*). In accordance with the greater breadth of this part of the Strait, the easterly surface-current flows at a much lower speed than in its narrower channel; its rate being reduced from nearly 3 miles to little more than $1\frac{1}{4}$ mile per hour. The use of the "current-drag" at 100 fathoms from the surface, in a part of the channel of which the depth was 147 fathoms, did not indicate any reduction in this rate; but a decided reduction was shown when the "drag" was lowered to 150 fathoms in a part of the channel of which the depth approached 200 fathoms (§ 67). As Capt. Calver deemed it inexpedient to lower the "current-drag" to a greater depth, since it would have been certain to foul against the rocky bottom, we were unable to ascertain by Mechanical means that the stratum of water *immediately* overlying that bottom has an *outward* movement; but whilst the existence of such an outflow may be regarded as a necessary inference from the existence of a powerful outward undercurrent at the opposite extremity of the Strait, valid evidence of it was afforded by the

fact that both the Temperature and the Density of the bottom-water brought up at two stations (Nos. 65 and 67), from 198 and 188 fathoms respectively, unmistakably indicated its derivation from the Mediterranean basin. Although its density corresponded rather with that of the 400 fathoms' stratum than with that of the 250 fathoms' stratum at the other end of the Strait, yet it may be very well conceived to be the water of the 250 fathoms' stratum reduced in density during its outward flow through the Strait by intermixture with the less dense water of the in-current.

117. It now no longer then admits of doubt that the water of the deeper part of the Mediterranean basin, which has undergone concentration by evaporation, is continually flowing outwards into the Atlantic, notwithstanding that in doing so it has to be brought nearer the surface, so as to pass over the ridge; and that the increase of density in the Mediterranean water, which would otherwise go on without check so long as the loss by evaporation is in excess of the fresh water returned into the basin, is thus kept within a very narrow limit.

118. The essential phenomena of the Gibraltar Current having been thus determined, we have to consider how they are to be accounted for; that is to say, to inquire (1) what is the power which gives motion to the enormous body of water continually flowing from the Atlantic into the Mediterranean; (2) what it is which not only gives motion to the undercurrent flowing from the Mediterranean to the Atlantic, but draws up the heavier water from the depths of the former to the comparative shallow of its limiting ridge; and (3) in what way the power is generated in each case.

119. These questions have been answered—as we believe correctly—by Captain Maury*, on the hypothetical assumption of the existence of an undercurrent, which has now been verified. He shows that in each case *Gravity* is the impelling power; and that in both cases this power originates from a common source—the excess of evaporation beyond the return of fresh water by rain and rivers, which produces at the same time a *reduction of the level*, and an *increase in the density*, of the water within the Mediterranean basin; the former drawing in surface-water by gravitation from the higher level outside, whilst the latter forces out deeper water by the excess of pressure of the superincumbent column. As the *vertical circulation* thus occasioned has not yet, so far as we are aware, been formularized under First Principles, and as these principles have a much more extended application than Capt. Maury himself seems to have supposed, we shall now present them in a systematic form.

120. The following appear to be self-evident propositions:—

I. That wherever there is a difference of *level* between two bodies of Water in free communication with each other, there will be a tendency towards the equalization of their levels by a *surface-flow* from the height towards the lower.

II. That so long as the difference of level is maintained, so long will this flow continue ; and thus any agency which permanently keeps the level of one body of water below that of the other (unless it directly antagonize the downward pressure of the higher water*), will maintain a permanent surface-flow from the higher towards the lower. This constant *tendency to equalisation* will keep the actual difference of level within very narrow limits.

III. That wherever there is a *want of equilibrium* arising from *difference of density* between two columns of water in communication with each other, there will be a tendency towards the restoration of equilibrium by a flow from the *lowest stratum* of the denser column towards that of the lighter, in virtue of the excess of pressure to which the former is subjected.

IV. That so long as the like difference of density is maintained, so long will this flow continue ; and thus any agency which permanently disturbs the equilibrium in the same sense, either by increasing the density of one column, or by diminishing that of the other, will keep up a permanent flow from the lower stratum of the denser towards that of the less dense.—This constant *tendency to restoration of equilibrium* will keep the actual difference of density within definite limits.

V. That if there be at the same time a *difference of level* and an *excess of density* on the side of the shorter column, there will be a tendency to the restoration of the *level* by a *surface-flow from the higher to the lower*, and a tendency to the restoration of the *equilibrium* by an *under-flow* in the opposite direction *from the heavier to the lighter* column.

VI. That so long as the *difference of level* and the *difference of density* are maintained, in the same sense, so long will each flow continue ; and thus a *vertical circulation* will be kept up by any continuous agency which alters at the same time both the *level* and the *density* of the two bodies of water,—provided that the *excess of density* is on the side of the *lower* column.

VII. That the rate of each flow, where it is not confined within definite limits, will depend simply upon the amount of disturbance, in the one case of *level*, and in the other of *density* ; and when this disturbance is small, it may be so slow as to be almost imperceptible, though not less real and effective. But if the communication between the two bodies of water take place through a long narrow channel, the rate of movement will increase so as to produce a decided current in each direction ; since the

* Thus it has been shown by Archdeacon Pratt, that in consequence of the local attraction produced by the high land of Asia, with nothing but Ocean to the southward, the sea-level at the mouth of the Indus is no less than 515 feet above that at Cape Comorin (Philosophical Transactions, 1859, p. 795). So, again, if Barometric pressure be *lower* over any Oceanic area than on other parts of the surface, there will be an *elevation* of the water-level in that area, equilibrium being reached when the *excess* of Water-pressure becomes equal to the *deficiency* of Air-pressure. (See Mr. T. G. Brent in Philos. Transact. 1867, p. 5.)

moving force will then act as a *constantly accelerating* one, until any further increase in rate is prevented by the opposing influence of friction, &c.

121. Now such an agency as that which is required by Principles VI. and VII. to maintain a double current in a narrow Strait actually exists in the cases of the Mediterranean and the Red Sea. It must be borne in mind in considering these, that whilst their basins are limited, the Ocean-basins at the other end of their respective Straits are practically unlimited; so that the levels and densities of the latter may be regarded as constant. Now as the excess of evaporation in the Mediterranean basin at the same time lowers the level and increases the density of the water which remains, the reduction of the level gives rise to a continual surface-inflow. But, on the other hand, the restoration of the level by an inflow of salt water, the density of the contents of the Mediterranean basin being already in excess, occasions a constant want of equilibrium between the columns of water at the two extremities of the Strait; and as the lighter water of the Atlantic cannot balance the heavier water of the Mediterranean, a portion of the latter is forced outwards as an undercurrent,—thus again producing a depression of the level, to be again restored by a surface-inflow from the Atlantic.—Thus the *original* moving force of both currents is the *heat of the Sun*.

122. The case may perhaps be made still plainer, by considering the effect of changes in its conditions. If the whole amount lost by the evaporation from the surface of the Mediterranean were replaced by the *fresh* water of rain and rivers, there would be neither lowering of its surface nor increase of its density; and there would be neither influx nor efflux through the Strait of Gibraltar. If, again, with the present excess of evaporation, the Atlantic were to supply *fresh* water instead of salt, the influx through the Strait of Gibraltar would be only that required to maintain the level, and thus to supply the loss by excess of evaporation; and as the columns at the two extremities of the Strait would remain in constant equilibrium, there would be no efflux. But as the water which flows in from the Atlantic is salt instead of fresh, and is itself rendered still more dense by concentration in the Mediterranean, the constantly renewed excess in the weight of the Mediterranean column can only relieve itself by as continual an efflux: this efflux, by lowering the surface-level, in its turn occasions an indraught to maintain it; and thus the in-current has to replace not only the fresh water lost by excess of evaporation, but also the denser water forced out by its excess of weight. These two agencies, like the perturbations of the Planets, are so balanced against one another, as to maintain a constant mean. If the evaporation were to increase, more Atlantic water would flow in; but the increase of density in the Mediterranean water would cause more of it to flow out, which again would occasion a larger indraught of the less dense water of the Atlantic. And thus the excess of density would be kept down to a very moderate amount,—as is actually *found to be the case* in the Red Sea, notwithstanding that the enormous

loss by evaporation from its surface is scarcely at all replaced by fresh water either from rain or rivers.

123. Now if it can be shown that a similar *vertical circulation* is maintained in the opposite direction, when the conditions of the case are altogether reversed, the explanation above given may, it is submitted, be regarded as having a valid title to acceptance. Such a converse case is presented by the Baltic, an inland basin which communicates with the German Ocean by three channels—the Sound, the Great Belt, and the Little Belt—of which the Sound is the principal. The amount of fresh water discharged into the Baltic is largely in excess of the quantity lost from its surface by evaporation; and thus its *level* would be continually *raised*, if it were not kept down by a constant surface-current, which passes outwards through the channels just mentioned. But the influx of fresh water *reduces* the *density* of the Baltic water; and as the water which the outward current is continually carrying off contains a large quantity of salt, there would be a progressive reduction of that density, so that the basin would at last come to be filled with *fresh* water, if it were not for a deeper inflow. Such an inflow of denser water might be predicted on Principle VI. as a Physical necessity, arising from the constant want of equilibrium between the *lighter* column at the Baltic end of the Sound and the *heavier* column at its outlet in the German Ocean; and that such an undercurrent *into* the Baltic has an actual existence, was proved two hundred years ago by an experiment of the same kind as that by which we have recently proved the existence of an undercurrent *out of* the Mediterranean. This experiment is cited by Dr. Smith (*loc. cit.*) in his discussion of the Gibraltar Current, as supplying an analogical argument for his hypothesis of the existence of an undercurrent in the Strait of Gibraltar; but he does not make any attempt to assign a Physical cause for the movement in either case*.—The condition of the Euxine is precisely parallel to that of the Baltic; and a surface-current is well known to be constantly flowing outwards through the Bosphorus and the Dardanelles, carrying with it (as in the case of the Baltic) a large quantity of salt. Now as the enormous volume of fresh water discharged into the Euxine by the Danube, the Dnieper, and the Don would in time wash the whole of the salt out of its basin, it is obvious that its density can only be maintained at its constant amount (about two-fifths that of ordinary sea-water) by a continual inflow of denser water from the Ægean,—the existence of which inflow, therefore, may be predicted on the double ground of *à priori* and *à posteriori* necessity.

GENERAL OCEANIC CIRCULATION.

124. The difference as to Level and Density between two bodies of sea-water, which produces the vertical circulation in the Strait of Gibraltar

* Prof. Forchhammer fully confirms Dr. Smith's statement; and further shows that the water which thus returns to the Baltic has the density of Sound water, the surface current being formed of the much lighter Baltic water.

and the Baltic Sound, may be brought about otherwise than by the excess of evaporation which maintains it in the one case, or by the continual dilution with fresh water which maintains it in the other. It may be easily shown that a constant and decided *Difference of Temperature* must have exactly the same effect. Let the Mediterranean basin be supposed to be filled with water of the same density as that of the Atlantic, and up to the same level; and to be then cooled down below the freezing-point of fresh water by the withdrawal of Solar heat, whilst the surface of the Atlantic continues to be heated, as at present, by the almost tropical sunshine of the Gibraltar summer. The cooling of the Mediterranean column, reducing its bulk without any diminution of weight, would at the same time lower its level and increase its density. An indraught of Atlantic water must take place through the Strait to restore that level; but this indraught would augment the weight of the column, giving it an excess above that of the column at the other end of the Strait; and to restore the equilibrium a portion of its deeper water must be forced out as an undercurrent towards the Atlantic, thus again reducing the surface-level of the Mediterranean. Now so long as the warm Atlantic water which comes in to maintain that level is in its turn subjected to the same cooling, with consequent lowering of level and increase of density, so long would the vertical pressures of the two columns, which would be speedily restored to equilibrium if both basins were subjected to the same heat or the same cold, remain in a constant state of inequality; and so long, therefore, on Principles V. & VI. (§ 120), must this *vertical circulation* continue.

125. Now the case thus put hypothetically has a real existence. For the Mediterranean cooled down by the withdrawal of Solar heat, let us substitute the Polar Basin; and for the Atlantic, the Equatorial Ocean. The antagonistic conditions of Temperature being constantly sustained, a constant interchange between Polar and Equatorial waters, through the seas of the Temperate Zone, must be the result. The reduction in the temperature of the Polar column must diminish its height whilst augmenting its density; and thus a flow of the upper stratum of Equatorial water must take place towards the Poles, to maintain the level thus lowered. But when the column has been thus restored to an equality of *height*, it will possess such an excess of *weight* that its downward pressure must force out a portion of its deeper water; and thus an underflow of ice-cold water will be occasioned from the Polar towards the Equatorial areas.

126. The agency of Polar Cold will be exerted, not merely in reducing the bulk of the water exposed to it, and thereby at the same time *lowering its level* and *increasing its density*, but also in imparting a *downward movement* to each new surface-stratum as its temperature is reduced, whereby a continual indraught will be occasioned from the warmer surface-stratum around. For the water thus newly brought under the same cooling influence will descend in its turn; and thus, as the lowest stratum will

be continually flowing off, a constant motion from above downwards will continue to take place in the entire column, so long as a fresh stratum is continually being exposed to the influence of surface-cold.

127. On the other hand, the agency of Equatorial Heat, though directly operating on only a thin film of surface-water, will gradually pump-up (so to speak) the Polar water which has reached its area by creeping along the deepest parts of the intermediate Oceanic basins. For since, as already shown, an indraught of the upper stratum surrounding the Polar basin must be continually going on, the place of the water thus removed must be supplied by water drawn from a still greater distance; and thus the movement will be propagated backwards, until it affects the upper stratum of the Equatorial area itself, which will flow off Pole-wards, bearing with it a large measure of Heat. The cold and dense Polar water, as it flows in at the bottom of the Equatorial column, will not directly take the place of that which has been draughted off from the surface; but this place will be filled by the rising of the whole superincumbent column, which, being warmer, is also lighter than the cold stratum beneath. Every new arrival from the Poles will take its place below that which precedes it, since its temperature will have been less affected by contact with the warmer water above it. In this way an *ascending movement* will be imparted to the whole Equatorial column, and in due course every portion of it will come under the influence of the surface-heat of the Sun. This heat will of course raise the level of the Equatorial column, without augmenting its absolute weight; and will thus add to the tendency of its surface-stratum to flow towards the lowered level of the Polar area. But as the *super-heating* extends but a short way down, and as the temperature of the water beneath, down to the "stratum of intermixture" (§ 80), is very moderate, whilst the water below that stratum is almost as cold as that of the Polar basin, it is evidently in the latter that the force which maintains this *vertical circulation* chiefly originates.

128. Here, then, we have a *vera causa* for a General Oceanic Circulation, which, being sustained only by the unequal distribution of Solar Heat, will be entirely independent of any peculiar distribution of Land and Water, provided always that this does not prevent the free communication between the Polar and Equatorial Oceanic areas, at their depths as well as at their surface. That this agency has been so little recognized by Physical Geographers, we can only attribute to the prevalence of the erroneous idea of the uniform deep-water temperature of 39° , of which the Temperature-observations made in our Expeditions of 1868 and 1869 have shown the fallacy. Until it is clearly apprehended that *Sea-water* becomes more and more dense as its temperature is reduced, and that it consequently continues to sink until it freezes, the immense motor power of Polar Cold cannot be apprehended; but when once this has been clearly recognized, it is seen that the application of *cold at the surface* is, in the case of Sea-water, precisely equivalent as a moving force

to the application of *heat at the bottom*, the motor power of which is universally admitted,—being practically utilized in keeping up the circulation through the hot-water Warming-Apparatus now in general use *. The movement thus maintained would not, on the hypothesis, be a rapid one, but a gradual *creeping* flow ; since the absence of limit would prevent the power which sustains it from acting as an *accelerating force*, as it would do if the Equatorial and Polar areas were connected only by a narrow channel, like the Atlantic Ocean and the Mediterranean Sea (§ 120, Princ. VII.).

129. That the Vertical Circulation here advocated on *à priori* grounds actually takes place in any mass of Salt water of which one part is exposed to surface-Cold and another to surface-Heat, is capable of ready experimental proof:—Let a long narrow trough with glass sides be filled with salt water ; and let heat be applied at one end (the Equatorial) by means of a thick bar of metal laid along the surface, with a prolongation carried over the end of the trough into the flame of a spirit-lamp ; whilst cold is applied at the other (the Polar) by means of a freezing-mixture contained in a metallic box made to lie upon the surface, or (more simply) by means of a piece of ice wedged in between the sides of the trough. A circulation will immediately commence in the direction indicated by the theory ; as may be readily shown by introducing some *blue* colouring liquid at the Polar surface, and some *red* liquid at the Equatorial surface. The blue liquid, as it is cooled, at once descends to the bottom, then travels slowly along it until it reaches the Equatorial end of the trough, then gradually rises towards the heated bar, and thence creeps along the surface back to the Polar end ; the red liquid first creeps along the surface towards the Polar end, and then travels through exactly the same course as the blue had previously done †.

130. That such a Vertical Circulation really takes place in Oceanic Water, and that its influence in moderating the excessive Cold of the Polar Areas and the excessive Heat of the Equatorial region is far more important than that of any surface-currents, seems to us a legitimate deduction from the facts stated in the Report of the ‘Porcupine’ Expedition for 1869. For, on the one hand, it was shown (§§ 116–118) that there is a general diffusion of an almost glacial temperature on the bottom of the deep Ocean-basins,

* The only scientific writer who has even approached what appears to us the truth on this point is Captain Maury, who has put forward the doctrine of a general interchange of water between the Equator and the Poles, resulting from a difference of Specific Gravity caused *inter alia* by difference of Temperature. But, as Mr. Croll remarks, “although Capt. Maury has expounded his views on the cause of Ocean-currents at great length in the various editions of his work, yet it is somewhat difficult to discover what they really are. This arises from the generally confused and sometimes contradictory nature of his hydrodynamical conceptions.” See Mr. Croll’s Paper “On the Physical Cause of Ocean-currents” in the Philosophical Magazine for October, 1870.

† This experiment has been exhibited, by the kindness of Prof. Odling, at the Royal Institution and at the Royal Geographical Society.

which, at depths exceeding 1000 fathoms, are occupied by Polar water, more or, less diluted by admixture according to the length of the course it has had to travel ; whilst between this stratum and that other stratum of warmer water which (on the hypothesis) is slowly moving Pole-wards, there is a "stratum of intermixture," in which there is such a rapid change of Temperature as might be expected from the relation of the upper and lower masses of water. This "stratum of intermixture" showed itself in a most marked manner in the Atlantic Temperature-observations of the present Expedition (§ 80) ; the descent of the Thermometer, which had been very slow with increase of depth between 100 and 800 fathoms, becoming suddenly augmented in rate ; so that between 800 and 1000 fathoms it fell *nine degrees*, namely from $49^{\circ} \cdot 3$ to $40^{\circ} \cdot 3$.

131. On the other hand, it was shown in the previous Report (§§ 119–121) that there is evidence of the slow Pole-ward movement of a great upper stratum of Oceanic Water, carrying with it a warm temperature ; which movement cannot be attributed to any such local influences as those which produce the Gulf-stream or any other currents put in motion by *surface-action*. Of such a movement, it was contended, we have a marked example in that north-easterly flow which conveys the warmth of Southern latitudes to the West of Ireland and Scotland, the Orkney, Shetland, and Faroe islands, Iceland, Spitzbergen, and the Polar basin generally. This flow, of whose existence conclusive evidence is derived from observations of the Temperature of these regions, is commonly regarded as a prolongation of the Gulf-stream ; and this view is maintained not only by Dr. Petermann*, who has recently collected and digested these observations with the greatest care, but also by Prof. Wyville Thomson †, as well as by Mr. Croll ‡. Having elsewhere fully stated our objections to this doctrine, and discussed the validity of the arguments adduced in support of it §, we shall here only record the conclusions which a careful examination of the present state of our knowledge of the subject has led us to form :—

I. That there is no evidence, either from the Surface-temperature of the Sea or from the temperature of sea-bord Stations along the western coast of Southern Europe, that the Climate of that region is ameliorated by a flow of Ocean-water having a temperature higher than that of the Latitude,—the surface-temperature of the Mediterranean Sea, which is virtually excluded from all Oceanic Circulation, being higher than that of the eastern margin of the Atlantic in corresponding latitudes, and the Climate of sea-bord Stations on the Mediterranean being warmer than that of Stations corresponding to them in Latitude on the Atlantic Coast ; and this not merely in summer, but also in winter. This Oceanic region may therefore be designated the *neutral area*.

* Geographische Mittheilungen, 1870, p. 201.

† Lecture "On Deep-sea Climates," in Nature, July 28, 1870.

‡ Memoir "On the Physical Cause of Ocean-Currents," in Phil. Mag. Oct. 1870.

§ Proceedings of the Royal Geographical Society, for Jan. 9, 1871.

II. That the evidence of Climatic amelioration increases in proportion as we pass Northwards from the *neutral area*, becoming very decided at the Orkney, Shetland, and Faroe islands ; but that, as was shown by the 'Porcupine' Temperature-soundings of 1869, the flow of warm water which produces this amelioration extends to a depth of at least 700 fathoms.

III. That this deep stratum of Warm water can be shown, by the correspondence in the rate of its Diminution of Temperature with depth, to be derived from the neutral area to the south-west ; where, as is shown by the 'Porcupine' temperature-soundings of 1870, it is separated by a distinct "stratum of intermixture" from the deeper stratum that carries Polar waters towards the Equator.

IV. That the slow north-easterly movement of such a mass of water cannot, on any known Hydrodynamical principles, be attributed to propulsive power derived from the Gulf-stream ; the last distinctly traced edge of which is reduced to a stratum certainly *not exceeding* 50 fathoms in depth, and not improbably less.

V. That, on the other hand, this slow Pole-ward movement of the upper warm layer of the North Atlantic, down to the "stratum of intermixture," is exactly what might be expected to take place as the *complement* of the flow of glacial water from the Polar to the Equatorial area, the two movements constituting a General *vertical* Oceanic Circulation.

VI. That there is a strong probability that the quantity of Water discharged by the Gulf-stream has been greatly over-estimated, in consequence of the rate of the surface-current having been assumed as the rate of movement through the whole sectional area, which is contrary to all analogy ; whilst there is also a strong probability that there is a *reverse* undercurrent of *cold* water through the Narrows, derived from the Polar current that is distinctly traceable nearly to its mouth. The upper stratum of this southerly current comes to the surface between the Gulf-stream and the coast of the United States ; whilst its deeper and colder stratum underlies the Gulf-stream itself*.

VII. That there is a strong probability that the quantity of Heat carried off by the water of the Gulf-stream has been greatly over-estimated ; the Temperature-soundings taken during the Cruise of the 'Porcupine' in the Mediterranean having shown that the very high temperature of the surface extends but a little way down, whilst the Temperature-observations in the Atlantic show that the descent into a cold stratum beneath

* That there is a slow southerly movement of Arctic water beneath the Gulf-stream is indicated by the fact that icebergs have been seen moving southwards in direct opposition to its surface-flow, their deeply immersed portion presenting a larger surface to the lower stratum than their upper part does to the more superficial layer,—as in the case of our "current-drag." And similar evidence is afforded by the southward drift of the buoy which was attached to the Atlantic Cable of 1865, but which broke away from it, apparently carrying with it a great length of the wire rope by which it had been attached.

may be very rapid. Hence the average of 65° assumed by Mr. Croll on the basis of observations made at considerable intervals of depth is altogether unreliable.

VIII. That the most recent and trustworthy observations indicate that the edge of the Gulf-stream to the north-east of the Banks of Newfoundland, is so thinned out and broken up by interdigitation with Polar currents, that its existence *as a continuous current* beyond that region cannot be proved by observations, either of Temperature or Movement.

IX. That the Gulf-stream and other local currents put in motion by the Trade-winds or other influences acting on the *surface* only, will have as *their* complement in a *horizontal* circulation return *surface*-currents; and that the horizontal circulation of which the Atlantic Equatorial Current and the Gulf-stream constitute the first part, is completed—so far as the Northern Hemisphere is concerned—partly by the direct return of one large section of the Gulf-stream into the Equatorial Current, and, as to the other section, by the *superficial* Polar currents, which make their way southwards, the principal of them even reaching the commencement of the Gulf-stream.

132. In conclusion it may be added that the doctrine of a General Vertical Oceanic Circulation is in remarkable accordance with the fact now placed beyond doubt by the concurrent evidence of a great number of observations, that whilst the Density of Oceanic water, which is lowest in the Polar area, progressively increases as we approach the Tropics, it again shows a decided reduction in the Intertropical area. It has been thought that an explanation of this fact is to be found in the large amount of rainfall, and of inflow of fresh water from great rivers, in the Intertropical region; but it is to be remembered that the surface-evaporation also is there the most excessive; so that some more satisfactory account of the fact seems requisite. Such an explanation is afforded by the doctrine here advocated, the Polar water which flows towards the Equator along the bottom of the ocean-basins being there (so to speak) pumped up and brought to the surface *. And it is not a little confirmatory of the views advanced in this Report, that in a recent elaborate discussion of the facts relating to the Comparative Density of Oceanic Water on different parts of the Earth's surface, the doctrine of a General Vertical Circulation is advocated as affording the only feasible rationale of them †.

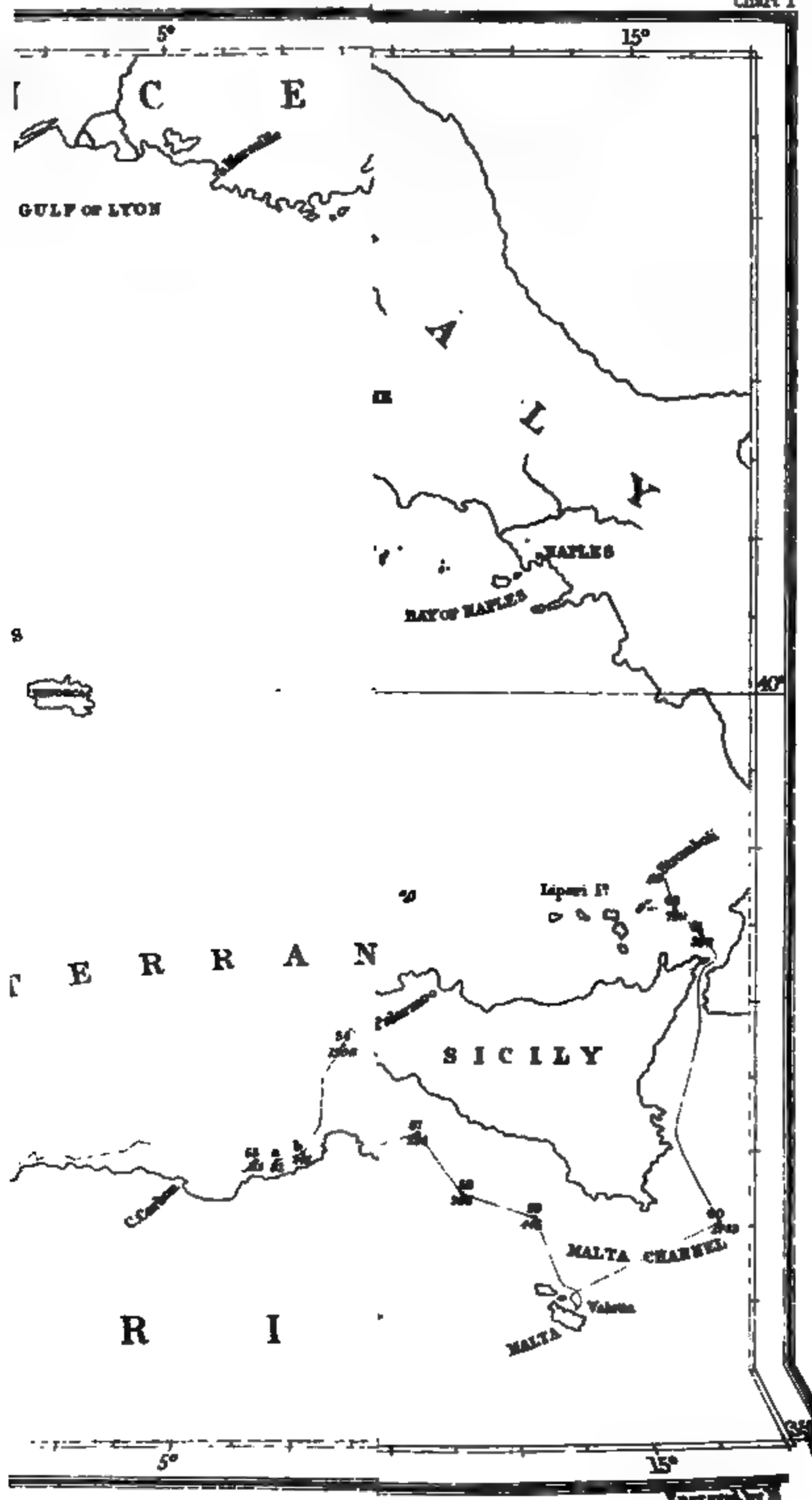
* That water of a *lower* should thus underlie water of a *higher* degree of Salinity, in travelling from the Pole to the Equator, is not difficult to account for, when the relative Temperatures of the two strata are borne in mind.

† “Densité Salure et Courants de l'Océan Atlantique,” par Lieut. B. Savy, *Annales Hydrographiques*, 1868, p. 620.

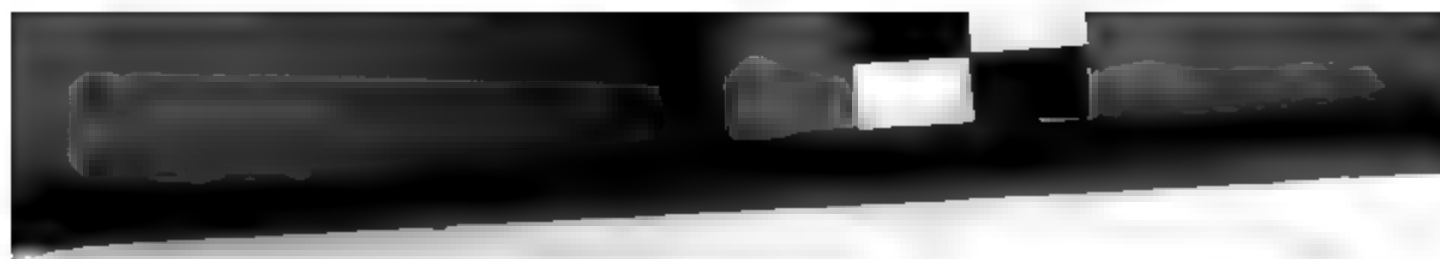
FIRST CRUISE OF THE 'PORCUPINE' (Chart I.).

Station No.	North Latitude.		West Longitude.		Depth in Fathoms.	Surface Temperature, ° Fahr.	Bottom Temperature, ° Fahr.
1.	48°	38'	10°	15'	567
2.	48	37	10	9	305	61.5	48.5
3.	48	31	10	3	690
4.	48	32	9	59	717	61.5	45.3
5.	48	29	9	45	100	62.3	51.5
6.	48	26	9	44	358	62.0	50.3
7.	48	18	9	11	93	61.0	51.3
8.	48	13	9	11	257	60.7	50.0
9.	48	6	9	18	539	64.0	48.0
10.	42	44	9	23	81	60.5	53.5
11.	42	32	9	24	332	60.5	51.5
12.	42	20	9	17	128	61.5	52.5
13.	40	16	9	37	220	64.5	52.0
14.	40	6	9	44	469	65.3	51.5
15.	49	2	9	49	722	67.5	49.7
16.	39	55	9	56	994	69.5	40.3
17.	39	42	9	43	1095	68.0	39.7
17a.	39	39	9	39	740	67.5	49.3
18.	39	29	9	44	1065	65.0	39.7
19.	39	27	9	39	248	64.7	51.7
21.	38	19	9	30	620	67.3	50.5
22.	38	15	9	33	718	66.3	52.0
23.	37	20	9	30	802	66.5	49.3
24.	37	19	9	13	292	67.5	52.7
25.	37	11	9	7	374	69.7	53.5
26.	36	44	8	8	364	71.7	52.7
27.	36	37	7	33	822	73.0	51.3
28.	36	29	7	16	304	71.5	53.8
29.	36	20	6	47	227	73.3	55.0
30.	36	15	6	52	386	73.0	52.7
31.	35	56	7	6	477	71.3	50.5
32.	35	41	7	8	651	71.5	50.0
33.	35	33	6	54	554	72.0	49.7
34.	35	44	6	53	414	71.7	50.0
35.	35	39	6	38	335	73.5	51.5
36.	35	35	6	26	128	75.0	55.0
37.	35	50	6	0	190	72.0	53.7
38.	35	58	5	26	503	71.7	54.0

Chart I







SECOND CRUISE OF THE 'PORCUPINE' (Charts I. and II.).

Station No.	North Latitude.	Longitude.	Depth in Fathoms.	Surface Temperature, ° Fahr.	Bottom Temperature, ° Fahr.
39.	35 59	5 27 W.	517	66.0*	55.5
40.	36 0	4 40 W.	526	74.5	55.0
41.	35 57	4 12 W.	790	74.5	55.0
42.	35 45	3 57 W.	790	74.0	54.0
43.	35 24	3 54 W.	162	74.7	55.0
44.	35 42	3 1 W.	455	70.0	55.0
45.	35 36	2 29 W.	207	72.7	54.7
46.	35 39	1 56 W.	493	73.5	55.5
47.	37 25	1 10 W.	845	69.5	54.7
48.	37 11	0 31 W.	1328	73.5	54.7
49.	36 29	0 31 W.	1412	71.5	54.7
50.	Algerine Coast	51	74.4*	
50a.			152		
50b.			510		
51.	36 55	1 10 E.	1415	75.0	54.7
52.	Algerine Coast	660	76.2*	
52a.			590		
53.	36 53	5 55 E.	112	77.0	55.5
54.	37 41	6 27 E.	1508	76.0	55.0
55.	37 30	6 51 E.	1456	70.5	55.0
56.	37 3	11 36 E.	390	78.0	56.5
57.	36 6	13 10 E.	224	76.8*	
58.	36 43	13 36 E.	266	75.5	56.5
59.	36 32	14 12 E.	445	76.5	56.5
60.	36 31	15 46 E.	1743	74.0	56.0
61.	38 26	15 32 E.	392	72.5	55.7
62.	38 38	15 21 E.	730	72.5	55.3
63.	Strait of Gibraltar	181	68.0	54.7
64.			460	65.6	54.7
65.			198	63.0	54.5
66.			147	69.0	Therm. lost
67.			188	73.0	55.3

* These temperatures are the averages of the day.

December 15, 1870.

General Sir EDWARD SABINE, K.C.B., President, in the Chair.

The Duke of Sutherland was admitted into the Society.

The reading of the Report on Deep-sea Researches carried on during the months of July, August, and September 1870, in H.M. Surveying Ship 'Porcupine,' by Dr. Carpenter, F.R.S., and Mr. J. Gwyn Jeffreys, F.R.S., was resumed and concluded.

December 22, 1870.

General Sir EDWARD SABINE, K.C.B., President, in the Chair.

The following communications were read :—

- I. "On the Extension of the Coal-fields beneath the Newer Formations of England; and the Succession of Physical Changes whereby the Coal-measures have been reduced to their present dimensions."

By EDWARD HULL, M.A., F.R.S., F.G.S., Director of the Geological Survey of Ireland. Received November 30, 1870.

(Abstract.)

In this paper the author, embodying with his own the observations of previous writers on the physical geology of Great Britain, especially those of Murchison, Godwin-Austen, Ramsay, Phillips, and the late Professor Jukes, showed that the Coal-measures were originally distributed over large tracts of England, to the north and to the south of a central ridge or barrier of Old Silurian and Cambrian rocks, which stretched across the country from North Wales and Shropshire into the Eastern Counties, skirting the southern margin of the South Staffordshire Coal-field. This barrier, or ridge, was a land-surface till the close of the Carboniferous period.

To the north of the central barrier, the highlands of Wales, the mountains of the Lake-district, and probably small tracts of the southern uplands of Scotland formed land-surfaces skirting portions of the Carboniferous area, while the Carboniferous tract to the south of the central barrier was probably bounded by a land-surface trending along the southern coast of England. The distribution of the Coal-measures at the close of the Carboniferous period was illustrated by a Map, No. 1.

It was then shown that the whole Carboniferous area was subjected to disturbances through the agency of lateral forces, whereby the strata were thrown into folds along axes ranging (approximately) in east and west directions; and as denudation accompanied and followed these disturbances, and acted chiefly over the arches (or anticlinals), large tracts were divested of Upper Carboniferous strata, and thus the first phase in the marking out of the limits of our present coal-fields was brought about. The effects of these movements and denudations were illustrated by Map No. 2.

The disturbances which ensued after the deposition of the Permian

strata, and which produced the discordances of stratification between the newer Palæozoic and Mesozoic formations, were shown to have acted along lines ranging approximately north and south, parallel to the axis of the Pennine Chain, and consequently in a direction transverse to those of the previous period. These disturbances were also accompanied by the denudation of strata from off the anticlinal arches, and the consequent dis-severance of the Coal-measure tracts over certain definite areas. The results of these movements (the second phase in defining the bounds of the coal-fields) were illustrated by Map No. 3.

From a consideration of the foregoing observations, the author came to the conclusion that the tendency of the British coal-fields to arrange themselves into the form of "basins" (sometimes partially concealed by newer strata), a tendency strongly insisted on by Prof. Ramsay, F.R.S., was due to the intersection of the two systems of flexures above described, one anterior to the Permian period, the other anterior to the Triassic period, and that the actual dis-severance of the coal-fields into basins was due to denudation acting with greatest effect along the anticlinal arches of these flexures.

The inference that the Yorkshire and Durham coal-fields are really basins rising to the eastward under the Mesozoic strata was drawn, an inference supported by the easterly rise of the Coal-measures along the sea-coast from the Coquet to the Tyne.

Guided by these principles, the author maintained that we are now in a position to determine with great accuracy the actual limits of the Coal-measures under the Mesozoic formations over the area to the north of the central barrier ridge (as indicated on Map No. 3); and that to the south of the ridge the application of the same principles would assist towards the solution of the question, though in a less degree, owing to the fewer opportunities for observation of the Palæozoic formations.

The author, however, concurred in the views advanced by Sir R. I. Murchison*, that in consequence of the great amount of denudation which the Carboniferous rocks had undergone over the area of the south of England previous to the deposition of the Mesozoic formations, little coal was to be expected to remain under the Cretaceous rocks.

II. "On the Constitution of the Solid Crust of the Earth." By the Ven. JOHN HENRY PRATT, Archdeacon of Calcutta, M.A., F.R.S. Received September 19, 1870.

(Abstract.)

In this paper the author applies the data furnished by the pendulum-observations recently made in India to test the truth of the following hypo-

* In his Address at the Meeting of the British Association at Nottingham, 1866. On the other hand, the views of Mr. R. Godwin-Austen, F.R.S., which tend rather in an opposite direction, should be well weighed by all who are interested in this question. (Quart. Journ. Geol. Soc. vol. xii.)

thesis regarding the Constitution of the Earth's Crust, which he propounded in 1864, viz.: that the variety we see in the elevation and depression of the earth's surface in mountains and plains and ocean-beds has arisen from the mass having contracted unequally in becoming solid from a fluid state; and that below the sea-level, under mountains and plains, there is a deficiency of matter approximately equal in amount to the mass above the sea-level; and that below ocean-beds there is an excess of matter approximately equal to the deficiency in the ocean when compared with rock; so that the amount of matter in any vertical column drawn from the surface to a level surface below the crust is now, and ever has been, approximately the same in every part of the earth.

In order to make this hypothesis the subject of calculation, the author takes the case of the attenuation of matter in the crust below mountains and plains, and the excess of matter below ocean-beds, to be *uniform* to a depth *m* times the height above the sea-level or the depth of the ocean, as the case may be.

The results are shown in the following Table, in which the numbers are the last figures in the ratio of the differences of gravity to gravity itself, carried to seven places of decimals. The decimal point and ciphers are omitted for convenience.

Stations.	Differences of gravity.			
	Relative effects of local attraction deduced from pendulum-observations.	Residual errors after correction by the method of		
		Dr. Young.	This hypothesis.	
			$m = 50.$	$m = 100.$
<i>Indian arc stations.</i>				
Punnæ.....
Bangalore	+384	−562	− 78	−557
Damargida	−323	−926	−455	−584
Kalianpur	+341	−208	+338	+315
Kaliana	−707	−957	+ 69	+320
<i>Coast stations.</i>				
Punnæ.....
Alleppy	+302	+314	+331	+360
Mangalore	−166	−154	−122	− 79
Madras	−197	−192	−138	− 78
Cocanada	+142	+153	+216	+291
<i>Ocean station.</i>				
Minicoy Island	+894	+906	+ 31	+102

The author points out from this Table that Dr. Young's, or the usual method of correction for local attraction, so far from improving matters, introduces very large residual errors of the arc and ocean stations; and, at places on the arc of meridian, all lying on the same side with reference to Punnæ. He observes that neither the usual method nor his own much

affects the coast stations; and attributes this to the want of more complete knowledge of the contour of the surface, both above and below the sea-level, in these parts. But his own method, in the case $m=50$, remarkably reduces the effects of local attraction at stations on the arc of meridian and out at sea (in Minicoy, an island 250 miles west of Cape Comorin or Punnæ); for the sensible negative quantity at Damargida and positive quantity at Kalianpur indicate a deficiency of matter below the first and an excess below the second, which exactly tally with the results independently brought out by relative deflections of the plumb-line as obtained by the survey: and the two large and most important effects, negative at Kaliana and positive at Minicoy, may be said to be almost annihilated by this method of correction. This last case of an *excess* of gravity out at sea (where the surrounding ocean has a deficiency of matter) being explained by his method, he regards as a very strong argument in its favour. And he finishes by saying that if his method is thus far successful in the particular supposition of the distribution below, whether in excess or defect, being *uniform*, which is most likely not strictly the case, there is every reason for concluding that pendulum-observations give support to the hypothesis regarding the Constitution of the Earth's Crust, when viewed on a large scale, admitting of local peculiarities, like the deficiency of matter near Damargida and the excess near Kalianpur, and the similar deficiency near Moscow.

III. "Actinometrical Observations made at Dehra and Mussoorie in India, October and November 1869, in a Letter to the President." By Lieut. J. H. N. HENNESSEY. Communicated by the President. Received September 7, 1870.

Mussoorie, July 22, 1870.

MY DEAR SIR,—In continuation of my last communication, dated April 25, 1870, I have now the pleasure to forward the actinometrical observations taken, during portions of October and November 1869, with the instruments of the Royal Society, and in compliance with the suggestions which the Committee of the Society made for my benefit.

(2) The two actinometers are of the kind invented by the Rev. G. C. Hodgkinson, and described by him in the Proceedings of the Royal Society, No. 89, vol. xv. Further description or allusion is therefore unnecessary, unless I add that the instrument is easily and accurately worked after but moderate practice, and that it is little liable to accident if rolled up in a padded sheet and packed within its own metal tube. It, however, imposes sensible drawbacks, from the delays incurred in throwing off a *suitable* amount of fluid into the chamber; and as this adjustment becomes deranged by any considerable alteration in the radiation, it is impossible to

continue a series of observations for any lengthened period (as, say, two hours) without introducing breaks of several minutes in its continuity.

(3) The two instruments used have been lettered by the observers A and B. The glasses, too, have been suitably marked as suggested by Sir John Herschel in the 'Admiralty Manual of Scientific Enquiry.' Actinometer B was used at Dehra by Mr. W. H. Cole, M.A., whose observations in sun and shade number 405 in all. The observations at Mussoorie were made by myself with actinometer A; they are 315 in number. And as respects the chronometers, barometers, and thermometers employed, I need hardly add that these instruments were of a superior order and well verified, or that they are in ordinary use at the head-quarters of Colonel Walker, R.E., Superintendent of the Great Trigonometrical Survey of India.

(4) And with every facility for reading the last-named instruments, I regret having omitted to arrange for a more frequent reading of the barometer, and that the wet- and dry-bulb thermometers were not recorded. It, however, so happened that during the days of observation in October and November last the sky was beautifully clear, with the trifling exceptions noted in the records of observations; and there is no reason to suppose that any sudden changes occurred in the hygrometric conditions of the atmosphere. In future, however, when the actinometers can again be worked, more numerous readings of the barometer and thermometers will be duly recorded.

(5) The stations of observation were Mussoorie and Dehra. The direct distance between them is about nine miles. The former stands on one of the southernmost ranges of the Himalayas; the latter is in the valley Dehra, or Dehra Dhoon. The hypsometrical elements for these stations, given in the result-abstract and elsewhere, are taken from the records of the Trigonometrical Survey of India. It appears from these values that Mussoorie station is above that at Dehra about 4700 feet.

(6) The procedure agreed on between Mr. Cole and myself was to observe daily a simultaneous series at 11^h 40^m A.M. (mean of all the observed times), another series at noon, and a third at 0^h 20^m P.M., the reckoning being in apparent time. The slight deviations from these times which appear in the result-abstract are due to little accidental causes almost inseparable from simultaneous work. After four days of these series, each observer was to determine the amount of heat stopped by the glass of his instrument employed. In these experiments I was too busy otherwise to reciprocate Mr. Cole's observations of November 1. On the 3rd of November, however, we both observed the intended succession of groups (nearly); so that several of these are made to discharge a double duty, and are introduced in the after-discussion of relative radiation. On November 4 we each obtained a complete hourly series about the hours of 8 A.M. to 4 P.M. These results terminated for the time the reciprocal series of observations Mussoorie-Dehra. Subsequently, in April 1870, when we were

both at Dehra, we carefully compared the two actinometers A and B together. This was the only occasion during all our observations when light clouds occasionally passed over the sun. But as the two instruments were set up within 3 or 4 feet of one another, and as we both used the same chronometer and read our scales at the same instant of time, there appears no reason why the results should not be accurate, relatively speaking.

(7) The constants thus determined are as follows :—

Actinometer A { Factor No. 1, to convert readings
with glass on into readings
glass off. } = 1.09 { Obtained from six groups
glass off and five groups
glass on, comprising sixty-
five observations in all.

„ B Factor No. 2, „ „ = 1.04 { Obtained from two sets of
observations, each con-
sisting of four groups
glass off and three glass on,
and comprising ninety-
six observations in all.

Factor No. 3, obtained from comparisons between A and B comprising 112 simultaneous observations, of which the following is a result-abstract reduced to 32° Fahr. and expressed in tenths of A's scale (both glasses on):—

Apparent time.	A.	B.	B—A.	Apparent time.	A.	B.	B—A.
	Observed by J. H. N. H.	Observed by W. H. C.			Observed by W. H. C.	Observed by J. H. N. H.	
h m s				h m s			
11 26 0	819	832	13	11 44 0	788	806	18
0 8 0	828	830	2	0 25 0	804	817	13
0 51 0	758	781	23	1 51 30	794	816	22
2 11 0	786	794	8	2 31 0	666	681	15
Mean ...	798	809	12	Mean ...	763	780	17

Whence $\frac{\text{mean A}}{\text{mean B}} = \frac{780.5}{794.5} = 0.982.$

We may also deduce

(W. H. C. at B) — (J. H. N. H. at A) = 11.

(J. H. N. H. at B) — (W. H. C. at A) = 17.

The accordance of these two average differences shows that no sensible “personal equation” appeared to exist between the observers.

(8) The observations simultaneous at Mussoorie and Dehra were, in the first instance, separated into groups, and combined group by group for a result. Subsequently groups were formed so as to include all the observations taken, subject to the following conditions :—

Seven (or fewer) sun-observations, with the intermediate observation in shade, were combined to produce one result.

Eight sun-observations, with the intermediate observations in shade, gave

groups of 5 and 4 sun-observations respectively (those in shade are here understood), the fifth sun being common to both groups.

Nine ditto, ditto, gave 5 and 5.

Ten ditto, ditto, gave 6 and 5.

Eleven ditto, ditto, gave 5, 4, and 4 ; and so on.

(9) The mean results by each group were next all corrected for excess of temperature above 32° Fahr., the Table of expansion for alcohol by Kopp, given in Gmelin's 'Chemistry,' being employed for this purpose. After this step the results by A were entered in the result-abstract Table and the corresponding values, in terms of *A glass off*, found by means of factor No. 1. The results by B were further corrected in the record of observations by means of factor No. 3. Being now in terms of *A glass on*, they were introduced into the result-abstract Table, and there reduced to *A glass off*, by means of factor No. 1.

(10) Thus the result-abstract Table contains the values obtained by each actinometer expressed in terms of *A glass on* as well as *glass off*. The latter values are those made use of in projecting the actinometric curves, and in the discussion of the observations. The former values will be useful should the Royal Society see fit to send me a third actinometer whose constant for reduction to the Kew standard has been duly ascertained. At present the required relation is wanting ; for though Professor Stokes was so exceedingly kind as to visit Kew with the object of getting the actinometers A and B verified, the necessary observations could not be made from want of time.

(11) Turning, now, to the diagram of actinometric curves and to the result-abstract Table, it is readily seen that the solar radiation decreases from some time about apparent noon both towards sunrise and sunset. This hour-angle change is least perceptible for some ± 1 hour (or less) from noon—a condition which indicates that observations for relative or absolute intensity are most valuable when made during this interval. Indeed even desultory observations might acquire importance by being restricted to these hours, the absence of cloud, mist, haze, or other abnormal interposition being always supposed.

(12) But besides the hour-angle change, the intensity is liable to rise and falls brought about in only a few minutes of time. Any observer who has used the instrument could venture to affirm that these fluctuations are not due to fallibility of observation. Whether their magnitude varies with that of the intensity or otherwise may be a matter of interest to ascertain ; and to this end series of observations, continued for as long a period as the construction of the instruments will permit, appear desirable.

(13) Again, there is a change of intensity from day to day, apparently not due to alteration in the sun's declination, so that the average *daily* curve (about noon) is higher or lower without any visible reason. It is interesting to notice that this daily change was common to Mussoorie and

Dehra. The two stations, it will be remembered, are about nine miles apart, and situated nearly on a common meridian.

(14) Collecting the results of observations, we obtain the following from simultaneous groups only :—

TABLE I.

		M *	D *	M — D.			M.	D.	M — D.	
27th.	h m s				3rd.	h m s				
	11 49 30	1007	876	131		11 14 30	}	1001	930	71
	0 9 30	968	904	64		0 15 0				
	0 29 30	941	888	53		11 46 0		1013	912	101
Mean	972	889	83	0 45 0	1006	908		98		
28th.	11 40 0	955	887	68	4th.	0 0 0	1015	929	86	
	0 0 0	969	883	86		0 15 0	1005	924	81	
	0 21 30	928	884	44		0 30 0	1020	937	83	
	Mean	951	885	66		0 45 0				
29th.	0 3 30	} 984	895	89		Mean	1010	923	87	
	0 4 30					8 3 30	847	718	129	
	0 21 30					9 11 0	943	821	122	
	Mean					975	901	74	10 8 0	996
30th.	11 40 0	980	912	68		11 10 0	}	1000	909	91
	0 1 30	977	903	74		0 11 0		986	913	73
	0 18 30	983	919	64		0 6 30		984	892	92
	Mean	980	911	69		1 8 0		}	964	845
				2 10 30	911	758	153			
				0 9 30	775	540	235			
				3 9 30						
					4 9 30					
					Mean from	}	990	905	85	
					11 ^h 10 ^m to					
					1 ^h 8 ^m					

Collecting these average daily results for ± 1 hour (about) from noon, we have :—

TABLE II.

	M.	D.	M—D.
1869. Oct. 27.	972	889	83
28.	951	885	66
29.	975	901	74
30.	980	911	69
Nov. 3.	1010	923	87
4.	990	905	85
Mean	980	902	77

and $\frac{M}{D} = 1.086.$

(15) In Tables I. and II. I have availed myself of none but actually simultaneous observations. We may, however, include every result at either station, provided the curve at the other station exists for the required time.

* M stands for Mussoorie, D for Dehra.

Thus at Dehra, October 28, we have the result 880 *observed*. The corresponding result at Mussoorie (*i. e.* at 11^h 15^m) is found from the diagram to be 945. Proceeding in this manner, and taking averages of all results within 5 minutes of one another to make one result, we find :—

TABLE III.

		M.	D.	M—D.			M.	D.	M—D.
27th.	h m s				3rd.	h m s			
	11 49 30	1007	876	131		11 15 45	1002	927	75
	0 9 53	969	904	65		11 30 0	1008	918	90
	0 29 30	941	888	53		11 36 30	1010	916	94
28th.	Mean	972	889	83	4th.	11 45 30	1013	912	101
	11 15 0	945	880	65		0 0 0	1006	908	98
	11 20 0	943	881	62		0 15 0	1015	929	86
	11 41 30	956	890	66		0 30 0	1005	924	81
	0 1 18	967	876	91		0 45 0	1020	937	83
	0 21 30	928	884	44		Mean	1010	921	89
29th.	0 33 30	937	885	52		8 3 30	847	718	129
	Mean	946	883	63		8 13 0	865	739	126
	0 3 45	984	897	87		9 11 23	943	823	120
	0 21 30	965	907	58		10 8 45	996	870	120
	0 32 0	964	895	69		11 9 45	1001	905	96
	0 41 0	962	900	62		11 19 0	990	910	80
30th.	1 20 0	958	891	67		0 8 0	987	913	74
	1 33 30	954	889	65		1 8 45	984	894	90
	Mean	965	897	68		2 10 0	964	845	119
	11 41 48	981	917	64		3 9 30	911	758	153
	0 1 30	978	904	74		4 8 0	780	546	234
	0 17 35	985	918	67		Mean from 11 ^h 9 ^m to 1 ^h 8 ^m ...	991	906	85
	Mean	981	913	68					

And collecting the average daily results of Table III. for ± 1 hour from noon, we obtain :—

TABLE IV.

	M.	D.	M—D.	
1869. Oct. 27.	972	889	83	
28.	946	883	63	
29.	965	897	68	
30.	981	913	68	
Nov. 3.	1010	921	89	
4.	991	906	85	
Mean	978	902	76	and $\frac{M}{D} = 1.081,$

which values of M—D and $\frac{M}{D}$ are practically identical with those of Table II.

(16) As regards the complete day-curve observed on November 4, it

appears that while the radiation at both stations *increases* from 8 A.M. and 4 P.M. towards some time about noon, the difference M—D *diminishes*. In other words, the radiation at the lower station increases more rapidly than at the upper ; and while at both stations the change is more rapid in the afternoon than in the forenoon, the relative change between forenoon and afternoon is greatest at the lower station.

(17) Mr. Hodgkinson, in his paper already referred to, quotes certain numbers obtained by Principal Forbes from his “ free hand curve ” of observations on the Faulhorn and Brienz, showing the relative intensity of the two stations. Calling his ratio $\frac{F}{B}$, the following may be contrasted :—

By Mr. Forbes.		From Table III., Nov. 4th, 1869.			
Hour.	$\frac{F}{B}$	h m s			$\frac{M}{D}$
		8	8	15	1'174
9	1'141	9	11	23	1'146
10	1'214	10	8	45	1'137
11	1'345	11	14	22	1'097
12	1'219	0	8	0	1'081
1	1'078	1	8	45	1'101
2	1'207	2	10	30	1'141
3	1'217	3	9	30	1'202
		4	8	0	1'429

where the heights of the stations are :—

	feet	
Faulhorn	8747	} by Principal Forbes.
Brienz	1903	
Mussoorie	6937	} from the records of the Great Trigonometrical
Dehra	2229	
		} Survey of India.

In conclusion I gladly acknowledge that I am much indebted to my friend Mr. Cole, not only for his skill and industry in taking the observations at Dehra, but for his cordial cooperation in reducing and discussing them.

Hoping you continue in the enjoyment of good health, I am, dear Sir, with kind wishes,

Yours very truly,
J. H. N. HENNESSEY.

General Sir Edward Sabine, K.C.B., &c.,
Pres. Roy. Soc., London.

The record of actinometer observations has been posted in a separate Packet *. The papers enclosed with this letter are the paper of actinometric curves and the result-abstract Table.

[* This record is preserved in the Archives for reference.—G. G. S.]

Result-abstract of Actinometrical Observations at Mussoorie on an outer range of the Himalayas, and at Dehra in the Dehra Dhoon, made by J. H. N. Hennessey, Esq., and W. H. Cole, Esq., M.A., of the Great Trigonometrical Survey of India.

Observations.			Mean results at Mussoorie observed with A and reduced to 32° Fahr.			Mean results at Dehra observed with B and reduced to 32° Fahr.		
In ☉	In X	Mean of the apparent times.	Nos.	In terms of A, glass on.	In terms of A, glass off.	Nos.	In terms of A, glass on.	In terms of A, glass off.
October 27, 1869.								
3	2	11 49 30	2 5	924	1007	1 5	804	876
3	2	0 9 30	6 10	888	968	6 10	829	904
4	3	0 11 0	6 12	890	970			
3	2	0 19 30	13 17	863	941	11 15	815	888
4	3	0 31 0	13 19	878	957			
3	2	0 45 30	10 24	835	965			
4	3	1 7 0	25 31	869	947			
October 28, 1869.								
4	3	10 57 0	32 38	874	953			
6	5	11 15 0	16 26	807	880
4	3	11 20 0	39 45	865	943			
4	3	11 40 0	46 52	876	955	27 33	814	887
6	5	11 43 0	27 37	818	892
4	3	0 0 0	53 59	889	969	38 44	810	883
5	5	0 2 15	38 47	796	868
5	4	0 21 30	60 68	851	928	48 56	811	884
5	4	0 33 30	56 64	812	885
5	4	0 43 30	69 77	868	946			
October 29, 1869.								
6	5	10 56 0	65 75	822	896
5	4	11 9 30	75 83	816	889
5	4	11 21 30	83 91	821	895
6	5	11 43 0	92 102	827	901
6	5	0 3 0	103 113	825	899
3	2	0 3 30	78 82	903	984			
3	2	0 4 30	107 111	821	895
5	4	0 21 30	83 91	885	965	114 122	832	907
4	3	0 32 0	122 128	821	895
4	3	0 41 0	128 134	826	900
6	5	2 20 0	92 102	879	958			
5	4	2 33 30	102 110	875	954			
3	2	2 33 30	135 139	805	877
5	4	2 54 30	140 148	783	853
5	4	3 10 30	149 157	765	834
4	3	3 21 0	157 163	739	806
October 30, 1869.								
4	3	11 40 0	113 119	899	980	164 170	837	912
6	5	11 43 0	164 174	845	921
5	4	11 43 30	111 119	902	983			
6	5	0 0 0	120 130	899	980			
5	4	0 1 30	122 130	896	977	175 183	828	903
6	5	0 1 0	175 185	831	906
4	3	0 17 0	131 137	905	986			
3	2	0 18 30	133 137	902	983	186 190	843	919
6	5	0 23 0	186 196	842	918

Observations.		Mean results at Mussoorie observed with A and reduced to 32° Fahr.			Mean results at Dehra observed with B and reduced to 32° Fahr.		
	Mean of the apparent times.	Noa.	In terms of A, glass on.	In terms of A, glass off.	Noa.	In terms of A, glass on.	In terms of A, glass off.
November 1, 1869.							
	11 15 0	197 203	916
	11 28 30	204 208	859	936
	11 45 45	209 214	935
	0 0 0	215 221	848	924
	0 15 0	222 228	932
	0 30 0	229 235	856	933
	0 45 0	236 242	917
November 3, 1869.							
	11 14 30	138 142	1001			
	11 15 0	243 247	930
	11 17 15	243 250	923
	11 30 0	251 257	842	918
	11 36 30	143 147	927	1010			
	11 45 0	258 264	912
	11 46 0	148 154	1013			
	0 0 0	155 161	923	1006	265 271	833	908
	0 15 0	162 168	1015	272 278	929
	0 30 0	169 175	922	1005	279 285	848	924
	0 45 0	176 182	1020	286 292	937
	1 8 30	183 187	908	990			
	1 19 30	188 192	945			
	1 30 30	193 197	879	958			
	1 41 30	198 202	973			
November 4, 1869.							
	8 3 30	203 207	777	847	293 297	659	718
	8 8 0	203 213	789	860			
	8 18 0	298 304	688	750
	9 8 0	214 224	862	940			
	9 11 0	218 224	865	943	305 311	753	821
	9 15 30	305 317	763	832
	10 8 0	225 235	914	996	318 328	804	876
	10 9 30	318 330	804	876
	11 8 30	236 244	918	1001			
	11 9 30	331 343	828	903
	11 10 0	236 246	917	1000			
	11 11 0	333 343	834	909
	11 19 0	244 250	908	990			
	0 6 30	251 259	905	986	344 352	838	913
	0 9 30	251 263	906	988			
	1 8 0	264 274	903	984	354 364	818	892
	1 9 30	354 366	821	895
	2 9 30	367 379	775	845
	2 10 30	275 287	884	964			
	3 9 30	288 300	836	911	380 392	695	758
	4 6 30	301 309	720	785			
	4 9 30	301 313	711	775	393 405	495	540
	4 17 0	309 315	684	746			

Remarks.

	Lat. N.	Long. E.	Height, in feet, above mean sea-level.
Mussoorie	30 28	78 7	6937
Dehra.....	30 19	78 6	2229

January 12, 1871.

General Sir EDWARD SABINE, K.C.B., President, in the Chair.

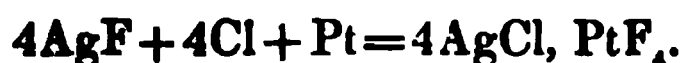
Prof. Benjamin Peirce (elected Foreign Member in 1852) and Col. J. T. Walker, R.E., were admitted into the Society.

The following communications were read :—

I. "On Fluoride of Silver.—Part II." By GEORGE GORE, F.R.S.
Received September 22, 1870.

(Abstract.)

This paper contains an exhaustive account of the behaviour of argentic fluoride in vessels of platinum, carbon, and various fluorides in contact with chlorine, bromine, and iodine at various temperatures. When argentic fluoride is completely decomposed by chlorine in platinum vessels at a red heat, the reaction agrees with the following equation :



Vessels of cryolite and of fluor-spar were found incapable of retaining argentic fluoride in a melted state. Other vessels were also made by melting and casting various mixtures of earthy fluorides at a high temperature ; and although forming beautiful products, probably capable of technical uses, they were not capable of retaining silver fluoride in a state of fusion. Numerous vessels were also made of seventeen different fluorides by moulding them in the state of clay and baking them at suitable temperatures ; these also were found incapable of holding melted fluoride of silver. Argentic fluoride was only superficially decomposed by chlorine at 60° Fahr. during thirty-eight days. When heated to 230° Fahr. during fifteen days in a platinum vessel in chlorine, it was very little decomposed. Chloride of silver heated to fusion in a platinum vessel in chlorine corroded the vessel and formed a platinum-salt, as when fluoride of silver was employed.

An aqueous solution of argentic fluoride agitated with chlorine evolved heat and set free oxygen, in accordance with the following equation :—



or



Dry hydrochloric acid gas completely decomposed argentic fluoride in a melted state, but only acted upon it superficially at 60° Fahr. A saturated aqueous solution of argentic fluoride was not precipitated by chloric acid.

Perfectly anhydrous fluoride of silver was only superficially decomposed by contact with bromine in a platinum vessel during thirty-six days at 60° Fahr., or during two days at 200° Fahr. At a low red heat in vessels of platinum, argentic fluoride was completely decomposed by a current

of bromine vapour, a portion of its fluorine being expelled and a portion corroding the platinum and forming an insoluble compound of fluoride of platinum and bromide of silver. In carbon boats at the same temperature the whole of the silver-salt was converted into bromide, the boat being corroded and the fluorine escaping in chemical union with the carbon. The action of bromine on an aqueous solution of argentic fluoride was similar to the action of chlorine. A solution of argentic fluoride yielded copious precipitates both with hydrobromic and bromic acids.

Under the influence of a temperature of 200° to 600° Fahr. in closed platinum vessels, iodine very slowly and incompletely decomposes argentic fluoride without corroding the vessels, and produces a feeble compound of argentic iodide, fluorine, and iodine, from which the two latter substances are expelled at a red heat. At a red heat in platinum vessels, iodine produces argentic iodide, and in the presence of free argentic fluoride corrodes the vessels in consequence of formation of platinic fluoride; iodine and fluorine pass away together during the reaction. In vessels of carbon at the same temperature argentic iodide is formed, the vessels are corroded, and a gaseous compound of fluorine and carbon is produced. By treating an aqueous solution of argentic fluoride with iodine, similar results are produced as with bromine and chlorine; a similar solution yields copious precipitates both with hydriodic and iodic acids.

A mode of analysis of iodine is also fully described in the paper. A known weight of iodine was dissolved in absolute alcohol, a strong solution of argentic nitrate of known strength added to it, in portions at a time, with stirring until the colour of iodine exactly disappeared. The mixture was evaporated, the free nitric acid expelled by careful heat, and the residue weighed. The residue was then heated to fusion, to convert the iodate of silver into iodide, and again weighed. Two experiments of this kind yielded accurate results, and the process was easy and expeditious.

II. "Some Experiments on the Discharge of Electricity through Rarefied Media and the Atmosphere." By CROMWELL FLEETWOOD VARLEY. Communicated by Prof. STOKES, Sec. R.S. Received October 5, 1870.

After the labours of Mr. Gassiot, one approaches this subject with diffidence, lest he should appear to be attempting to appropriate the glory which so justly belongs to that gentleman and to Professor Grove. The nature of the action inside the tube is at present involved in considerable mystery, but some light is thrown upon the subject by the following experiments. Before describing them, however, the author wishes to observe that he has seen Mr. Gassiot's last paper*, and finds that, so far as regulating the strength of the current is concerned, he has been proceeding in a similar manner to the author.

* Proceedings of the Royal Society, vol. xii. p. 329.





Varley

Proc Roy Soc Vol. XIX. Pl. II

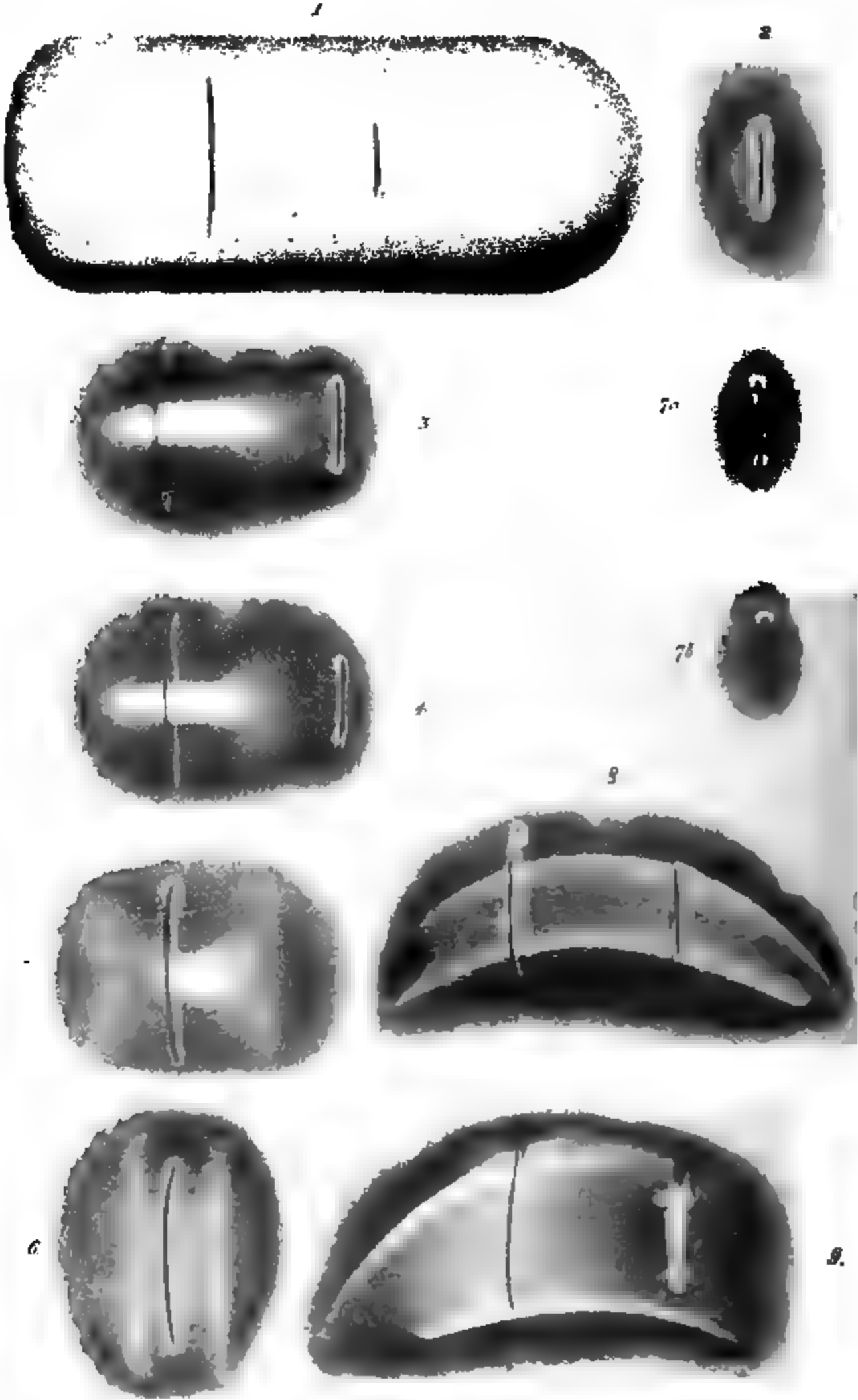


Fig. 1.

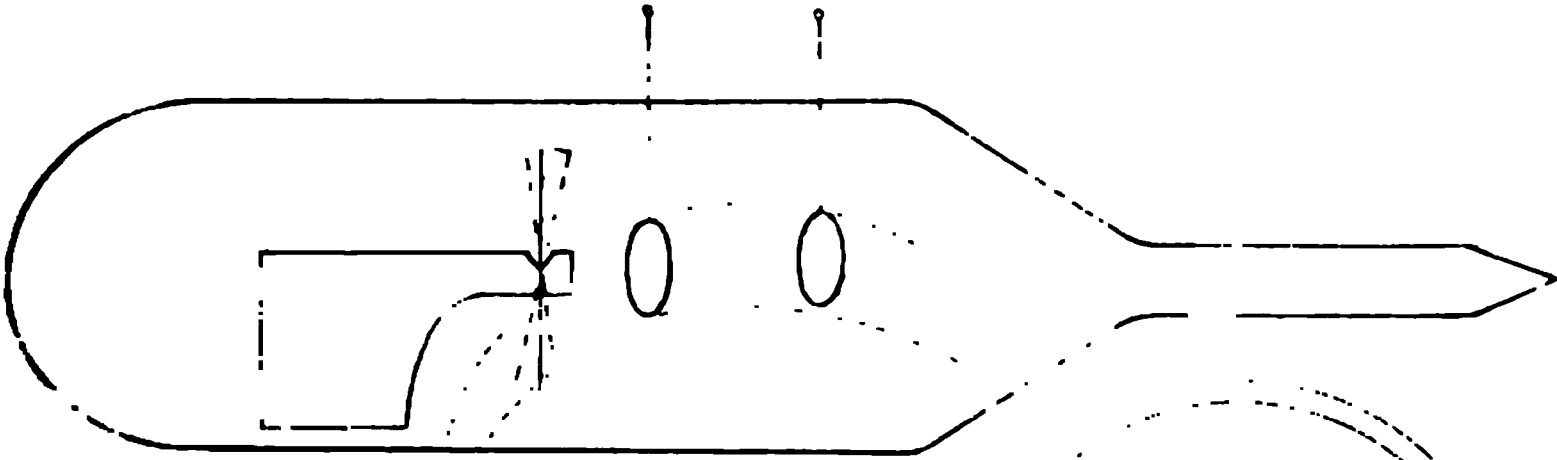
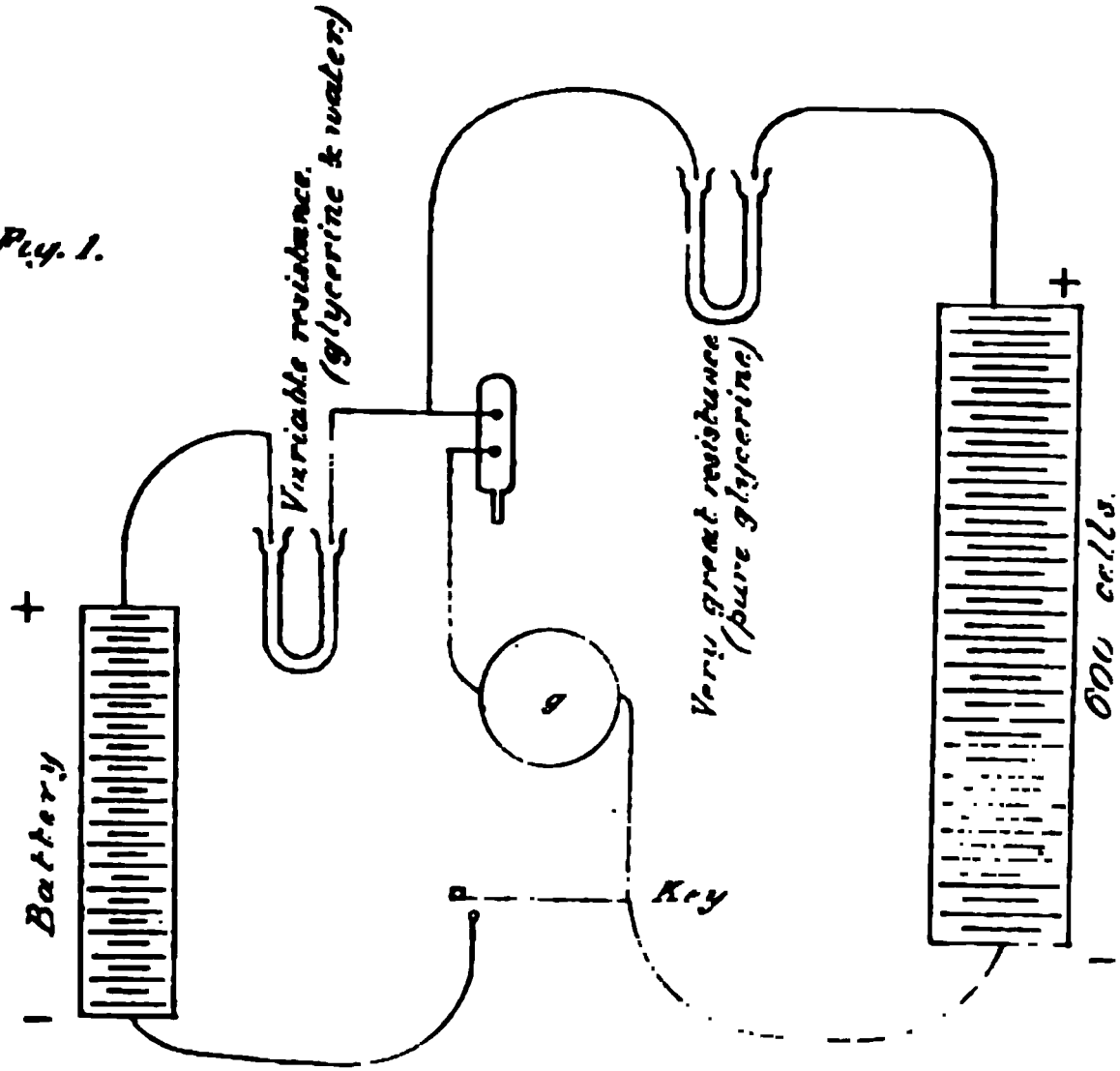


Fig. 2.

Section of tube, actual size, showing the tube supported by a silk fibre.

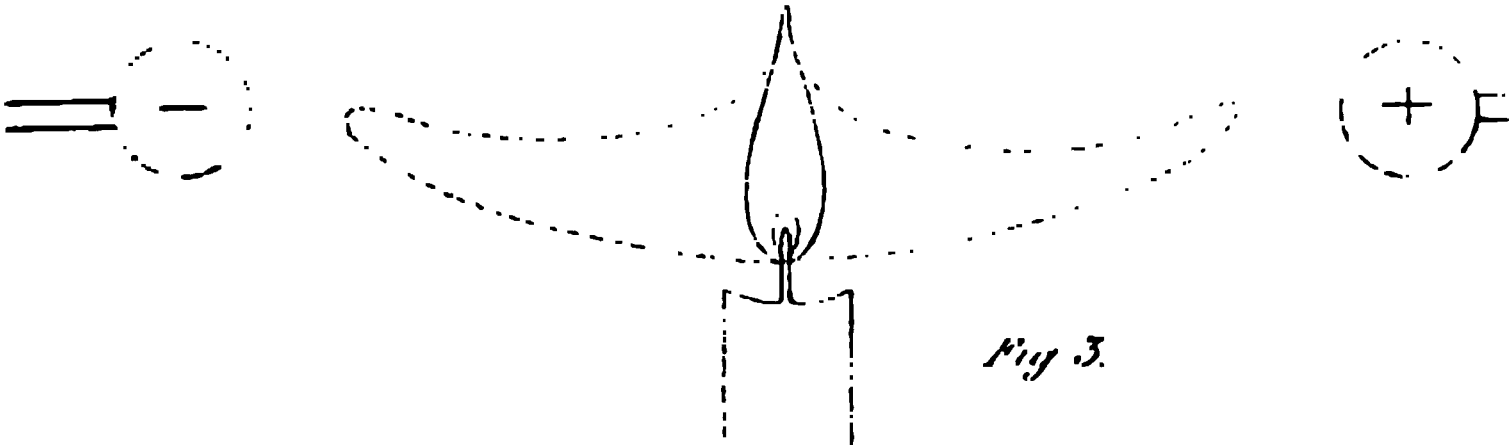
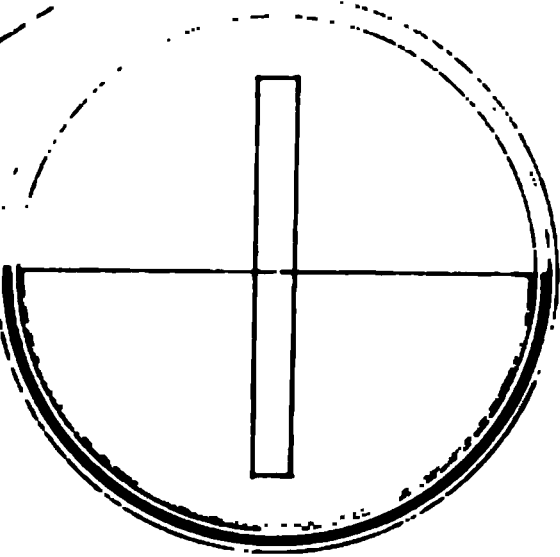
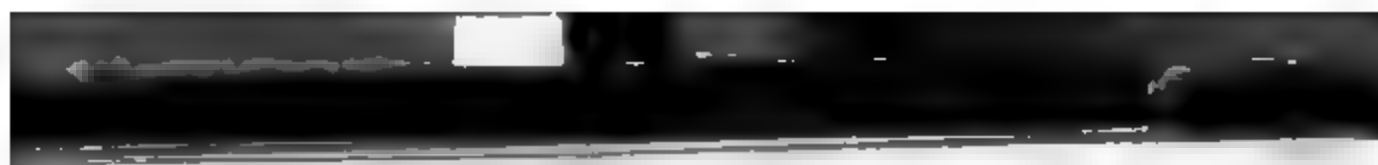


Fig. 3.



The tube principally used in these experiments is shown full size in photograph No. 1 (Pl. II.); it contains two aluminium wire rings, the one $\frac{4}{10}$ inch in diameter, the other $\frac{2}{10}$ inch, and separated $\frac{8}{10}$ inch: the tube was $1\frac{1}{4}$ inch in diameter, $3\frac{1}{2}$ inches in length; it was one of Geissler's manufacture, was very well exhausted, and professed to contain hydrogen. A U-shaped glass tube containing glycerine and water was placed in circuit. Two aluminium wires inserted in this tube gave a ready means of reducing or augmenting the resistance at pleasure. Glycerine affords an easy means of producing very great resistances.

The battery used in this experiment was a Daniell's battery, each cell having a resistance of from 50 to 100 ohms. The resistance of the glycerine-and-water tube was between 2 and 3 megohms; this latter resistance was made large, in order that the resistance of the tube and battery might be neglected without entailing error.

The following laws were found to govern the passage of the current:—1st, each tube requires a certain potential to leap across; 2nd, a passage for the current having been once established, a lower potential is sufficient to continue the current; 3rd, if the minimum potential, which will maintain a current through the tube, be P , and the power be varied to $P+1$, $P+2$, &c. to $P+n$, the current will vary in strength, as 1, 2, &c. n .

Tables I. & II. (p. 242) illustrate this; there is a little irregularity in the figures due to the irregularity of the battery, although it was recharged for the occasion.

It thus appears that a certain amount of power is necessary to spring across the vacuum; after that it behaves as an ordinary conductor, excluding that portion of the battery whose potential is P , and which is used to balance the opposition of the tube. In these experiments P was 304 cells. The tube in question could not be persuaded to allow a current of less than 323 cells to pass; but when once the current had established a channel, on lowering the potential by *short circuiting* portions of the battery, so as not to break the circuit, the current would flow when the battery was reduced to 308 cells. By, however, passing a current from 300 cells, in the manner shown in Pl. III. fig. 1, through the second tube, filled with pure glycerine, and offering several thousand megohms resistance, an extremely feeble current, too weak to affect the galvanometer, kept a channel open by its passage; with this arrangement the figures in Table II. were obtained, which are more regular at the commencement, and a power of $P+1$ would pass across the tube.

The positive pole alone was observed to be luminous when the current was very minute, and the negative only was luminous when the current was strong. The following experiments were tried, and the results, which have been photographed, accompany this.

A current was passed through the U tube and the vacuum; the U tube contained pure glycerine, and had a very large resistance, which was gra-

dually reduced. At the commencement it was more than 10,000 megohms; the upper or small ring was positive, the lower ring was negative.

The power was so reduced that the faintest possible light only was visible; in this case the positive wire alone was luminous, whether it were the large or small ring that was positive. No. 2 (Pl. II.) is a photograph of this. The light was so feeble that, though the experiment was conducted in a perfectly dark room, we were sometimes unaware whether the current was passing or not. An exposure of thirty minutes' duration left, as will be seen, a very good photographic record of what was taking place; this means of viewing light too feeble for the eye may receive other applications. The resistance was then reduced, when the light became much more brilliant,—a tongue of light projected from the positive pole towards the negative, the latter being still almost completely obscure.

The light around the positive pole was to all of our eyes white, while the projecting flame was a bright brick-red. This bright brick-red, however, possessed great photographic power, as will be seen by photograph No. 3. The negative wire at this stage began to show signs of luminosity.

As the power was increased, the flame became detached from the positive pole, as shown in photograph 4.

On still further increasing the power, the positive pole ceased to be luminous, as in photograph 5; and on still further increasing the power, by removing the U tube altogether, the phenomena presented themselves which are shown in photograph 6, in which the light surrounded the negative wire. The photograph shows a white flattened hour-glass, apparently detached from the wire; to the eye, however, the wire appeared to be surrounded by a bright blue envelope $\frac{1}{4}$ inch in diameter, which did not possess sufficient photographic power to leave a record of itself, while the red portion did so: this photograph was exposed only ten seconds to the light.

A large condenser was now attached to the battery, and discharged through the tube (the condenser had a capacity of 27 microfarads); this was equivalent to a momentary contact with a battery of little or no resistance. The flash was exceedingly brilliant to the eye: it could be heard outside the tube with a sharp click; the eye, however, was so dazzled as not to be able to see its shape.

Photographs 7 *a* and 7 *b* show that the light was confined entirely to the *positive pole*; thus, then, as the power is increased from nothing upwards, the first pole to become luminous is the positive; secondly, ~~the~~ two poles become luminous; thirdly, the negative pole alone is luminous; and fourthly, with an instantaneous discharge, the positive pole only ^{is} luminous. The eye and the collodion-plate do not, however, tell the same tale in photograph 6.

When the resistance in the U tube was greatly reduced, and a galvanometer (not very sensitive) was inserted, so that the chief resistance circuit was that of the exhausted tube, as the potential was augmented ~~ce~~ ^{the}

by cell, the changes took place abruptly and suddenly. For example, when the power was so low that the positive pole only was visible, the current was feeble, and kept augmenting in power as cell after cell was added on. Suddenly the luminous red flame (phot. 3) made its appearance, and the galvanometer showed that the current had suddenly augmented three or four times in power. As the power was again further increased cell by cell, the current again steadily augmented in proportion, until the luminous tongue suddenly disappeared, and the appearance in 6 was shown, the galvanometer showing a still further sudden increase in the current.

The phenomena shown in 4 and 5 can only be obtained by inserting a large resistance.

Nature of the luminous cloud.—Plücker has shown that when such an exhausted tube, with a current through it, is placed between the poles of an electro-magnet, a luminous arch is produced, which arch follows the course of the magnetic rays. (See photograph 8, in which the negative pole was a small ring. Photograph 9 shows the arch when the large ring was negative.)

As the electro-magnet is magnetized, the tube, which before was full of a luminous cloud, is seen gradually to change; the magnet gathers up this diffused cloud, and builds up the arch shown in 8 and 9.

Inasmuch as the electricity was passing in a continuous current from the battery, from wire to wire, it is evident the light is projected right and left into those parts of the tube where there is no electric current flowing.

To endeavour to ascertain the nature of this arch, a tube (Pl. III. fig. 2) was constructed. A piece of talc, bent into the form U, had a fibre of silk stretched across it; on this fibre of silk was cemented a thin strip of talc, 1 inch in length, $\frac{1}{10}$ inch broad, weighing about $\frac{1}{10}$ of a grain. The tube was sealed up and exhausted; carbonic acid and potash were used to get a high vacuum. When the magnet was not magnetized, the passage of the current from wire to wire did not affect the piece of talc. When the magnet was charged, and the luminous arch was made to play upon the upper portion of the talc, it repelled it, no matter which way the electric current was passing.

When the tube was shifted over the poles of the magnet so as to project the luminous arch against the upper part of the talc, the upper end of the arch was repelled in all instances; the arch, when projected against the lower part of the talc, being near the magnet, was more concentrated, and the angle of deviation of the talc was as much as 20° . When the upper end of the arch, which was much more diffused, was thrown upon the lower part of the talc, it was repelled about 5° .

This experiment, in the author's opinion, indicates that this arch is composed of *attenuated particles of matter projected* from the negative pole by electricity in all directions, but that the magnet controls their course; and

these particles seem to be thrown by momentum on each side of the negative pole, beyond the limit of the electric current.

This arch requires time for its formation, for when a charged condenser is discharged through the tube no arch is produced. The arch from the negative pole is a hollow cylinder; the little talc tell-tale against which the arch was projected cut out the light, and a corresponding dark space existed throughout the remainder of the course of the arch.

There was on the talc, at the spot where the arch struck it, a little bright luminous cloud, as though the attenuated luminous vapour were condensed by this material obstruction.

Great care had been taken not to let the arch strike the single filament of silk which suspended the talc. Having demonstrated that the talc was repelled as described, the arch was allowed to play against the silk fibre, which the author expected would have been instantly burnt; such, however, was not the case. Even when a powerful induction-coil replaced the battery, the fibre remained unhurt.

Comparison of the above Phenomena with Discharges between the Poles of a Holtz's Machine in air.

In the first part of this paper four different kinds of discharges were described *in vacuo*. With a "Holtz's" machine, which will give 11-inch sparks in the air, four well-marked different kinds of discharge have been obtained *in the air*; one of which, the author thinks, will explain the curious and rare phenomenon known as "ball lightning."

In the experiments hereafter referred to, the condensers were in all cases attached to the "Holtz's" machine. The first discharge is the long 11-inch zigzag spark or lightning-flash; the second is the well-known "brush," which is best obtained by connecting the negative pole of the "Holtz's" machine to the earth; the third kind of discharge is a hissing red flame, $\frac{1}{2}$ inch in length, playing about the negative pole, the positive pole being scarcely luminous at all, and if luminous, at one or two points only; the fourth or most remarkable phenomenon is best obtained in the following manner (it should be understood that the brass balls on each of the poles are about an inch in diameter):—Tie to the negative pole a small thin strip or filament of wood, 3 inches in length, and bent so as to project on each side of the negative pole, and a little beyond it towards the positive. On rotating the machine, two bright spots are seen upon the positive pole. If the positive pole be made to rotate upon its axis, the luminous spots do not rotate with it; if, however, the *negative pole*, with its filament of wood, be rotated, the spots on the *positive pole* obey it, and rotate also. The insertion of a non-conductor, such as a strip of glass, in front of the projecting wooden end, obliterates the luminous spot on the positive pole. When the author first discovered this, he, seeing apparently pieces of dirt on the positive pole, wiped it clean with a silk handkerchief, but there

they remained in spite of all wiping ; he then examined the negative pole, and discovered a minute speck of dirt corresponding to the luminous spots on the positive pole.

When the filament of wood is removed from the negative pole, there is sometimes a luminosity or glow over a large portion of the surface of the positive ball. If in this state three or four little pieces of wax, or even a drop or two of water, be placed upon the negative pole, corresponding non-luminous spots will be found upon the positive pole, which rotate with the former, but do not with the latter.

It is therefore evident that there are lines of force existing between the two poles, and by these means one is able to telegraph *from* the negative *to* the positive pole to a distance of 8 inches through the air, without any other conductor than that which the electrical machine has constructed for itself across the non-conducting gas.

The foregoing seems to the author to give a possible explanation of "ball-lightning." If it be possible for there to be a negatively electrified cloud sufficiently charged to produce a flash from the earth to the cloud, a point in the cloud would correspond to the wood projection on the negative conductor : if such a cloud exist, a luminous spot would be seen moving about the surface of the earth, corresponding to the moving point of cloud over it, and thus present phenomena similar to those described by the privileged few who have witnessed this extraordinary natural phenomenon.

The following experiment shows that, prior to the passage of the electric spark, a channel is prepared for this spark to pass.

The positive and negative balls of the machine were separated to a distance of 6 or 7 inches, and a common candle-flame was placed midway between them. On rotating the machine, the flame was drawn out on each side just prior to the passage of the spark, as shown in the accompanying sketch (Pl. III. fig. 3). Sometimes it extended to a width of 5 or 6 inches ; this took place every time the spark passed. It is well known that the duration of this spark is less than the $\frac{1}{100,000}$ part of a second ; the flame occupied the $\frac{1}{8}$ or $\frac{1}{10}$ part of a second in flying out to make the conducting channel through which the discharge went.

The author has been informed more than once, by captains of vessels, that when men have been struck by lightning a burn has been left upon the skin of the same shape as the object from which the discharge flew. In one instance he was informed that some brass numbers attached to the rigging, from which the discharge passed to the sailor, were imprinted upon his skin.

It is now seen that this is perfectly possible if the discharge be a negative one—that is, if the man be + to the brass number.

TABLE I.

1.	2.				3.	4.
Cells of Daniell's Battery, P+n.	Observed Deflections of Reflecting Galvanometer.				Mean.	3rd Col. divided by n.
307=P+ 3	0	0	0	0	0	0
308=P+ 4	5	5½	5	5½	5½	1.3
309=P+ 5	9	9	9	9	9	1.8
310=P+ 6	12	12½	12	12½	12½	2.04
311=P+ 7	14	14	14	2
312=P+ 8	16	16	2
313=P+ 9	17½	18	18	..	17½	1.97
314=P+10	19½	19½	19½	1.95
315=P+11	21½	21	22	21½	21½	1.95
316=P+12	23½	23½	23½	..	23½	1.96
317=P+13	25½	25½	1.96
318=P+14	27½	27½	1.97
319=P+15	29½	29½	1.97
320=P+16	31½	31½	31	31	31½	1.95
*323=P+19	37½	38	37	37½	37½	1.97
325=P+21	40½	41	40½	..	41½	1.94
330=P+26	51	51	51	1.96
335=P+31	60½	60½	60½	1.95
340=P+36	70	70	70	1.94
345=P+41	79½	79½	79½	1.94
350=P+46	89	89	89	1.94
355=P+51	98½	98½	98½	1.93
360=P+56	108	108	108	1.93
365=P+61	118	118	118	1.94
370=P+66	128	128	128	1.94
375=P+71	139	140	139½	1.96
380=P+76	150	150	150	1.97
TABLE II.						
304=P+ 0	0	0	0
305=P+ 1	2	2	■	2
306=P+ 2	4	4	■	2
307=P+ 3	6	6	0	2
308=P+ 4	8	8	8	2
309=P+ 5	10	10	10	2
310=P+ 6	12	12	12	2
320=P+16	31½	32	31½	1.97
330=P+26	51	51	51	1.96
340=P+36	71	71½	71	1.97

* This power (323) was the lowest at which the current would jump.

III. "Polarization of Metallic Surfaces in Aqueous Solutions, a new Method of obtaining Electricity from Mechanical Force, and certain relations between Electrostatic Induction and the Decomposition of Water." By CROMWELL FLEETWOOD VARLEY. Communicated by Prof. Sir W. THOMSON, F.R.S. Received October 5, 1870.

(Abstract.)

Platinum plates immersed in sulphuric acid and water, as in a decomposition-cell, require a potential of about 1·7 Daniell's cell to decompose the water; with potentials of less amount the platinum plates can be charged and discharged like condensers. They have enormous electrostatic capacity. Mercurial surfaces equally admit of polarization with hydrogen. A surface of mercury in dilute sulphuric acid, when made negative to the water by means of a powerful battery, flattens out. If the mercury be replaced by an amalgam of proper consistency, the flattening out is increased; the reversal of the current restores the amalgam to its original dimensions. By reversing the process, electric currents can be obtained from mechanical force.

A large vessel on a board has within it two shallow funnels, which are connected by means of glass tubes with similar vessels outside of the large one. Pure mercury is poured into the funnels until they and the outside vessels are one-third filled. By tilting the board, mercury runs into the one funnel and out of the other, and thus the surface in the one is made to increase while that in the other decreases. Dilute sulphuric acid is poured into the larger vessel so as to cover the two funnels; the latter are connected together through a galvanometer.

If the mercury be pure and free from polarization, the tilting of the board produces no electric current. On polarizing one of the surfaces with hydrogen by a battery, it gives rise to a current through the galvanometer, and thus shares the polarization over the two surfaces. If the battery be removed, on augmenting the one surface and diminishing the other, a current of electricity is seen to pass through the galvanometer.

A convenient method of showing this experiment on a large scale is to procure a gutta-percha trough 4 inches deep and 4 by 2 inches broad. A partition of the same material 2 inches high divides the lower half into two separate chambers: these are partly filled with mercury; amalgamated platinum plates, hung from a balance-lever, dip into the mercury. On depressing one set of plates the others are elevated, and thus the mercurial surface exposed to the fluid is alternately augmented and diminished to a large amount. Twelve of these arranged in series give a current of rather more potential than one cell of Daniell's battery when the mercury is polarized with hydrogen. The addition of a minute fragment of zinc to the mercury maintains the polarization for a very long time, and the power is considerably increased thereby. When a large surface of mercury (25 circular inches) has been polarized with a power of half a Daniell's cell and is rapidly reduced to the diameter of $\frac{1}{30}$ inch, by letting the mercury flow out

of the funnel, some bubbles of hydrogen gas appear just as the last of the mercury is running out, the decrease of surface evidently augmenting the potential sufficiently to decompose the water : floating a small piece of platinum on the mercury renders this phenomenon much more distinct.

All attempts to polarize the mercury with oxygen have failed to give a current. By depolarizing the mercury with a battery until no current is generated by varying the dimensions of the exposed mercury surface, a metallic surface neutral to the fluid is obtained.

The second part of the paper refers to the electrostatic capacity of platinum plates in dilute acid and water.

In order to determine this point, it is necessary to use sensitive, rapidly oscillating, reflecting galvanometers of very small resistance. The author has succeeded in measuring the charge which a square inch of platinum exposed to another square inch of platinum surface receives from potentials varying from 0.02 of a Daniell's cell up to 1.6 Daniell's cell. From a potential of 0.02 to 0.08 the capacity remains sensibly constant ; that is, the discharge from the plates varies directly as the potential. When the potential increases beyond 0.08, the charge which the plates receive increases in a greater ratio, the capacity being 3.3 (in one experiment) and 3.1 (in another experiment) times as great with a potential of 1.6 as it was with the potential of 0.1.

There is great difficulty attending accurate determination of the latter amounts ; but the author expects that this increase of capacity will be found to vary as the square root of the potential. The capacity of the platinum plates with varying powers is shown in the accompanying Tables.

The author thinks these experiments tend to show that the fluid does not actually touch the platinum plate, but is separated from it by a film, which film, if a pure gas, must be less than the $\frac{1}{1,000,000,000}$ part of an inch, when very small potentials are used. This distance decreases as the potential rises. Inasmuch as two surfaces equally electrified with the opposite electricities attract each other with a power varying inversely as the square of the distance, the experiment would seem to indicate that at very small distances the platinum repels the water with a power varying inversely as the cube of the distance.

The phenomena of electrification render accurate determinations of the capacity extremely difficult. The fact of the phenomena of electrification being present, leads the author to think that the separating film (if such a film exists) is not a pure gas, but has five or more times as much electrostatic capacity as pure gas.

A useful inference drawn from the above experiments is the impossibility of working through any considerable length of uninsulated wire in the ocean.

The French Atlantic cable from Brest to St. Pierre works, upon the average, ten words per minute ; the author calculates that a solid conductor of the same weight per mile as that used between the above stations must be reduced to a length of less than 1100 yards in order that the rate of signalling through it shall be not slower than through 2500 miles of

the same conductor insulated; and the bare wires can only be practically worked on circuits not exceeding a mile.

TABLE I.

Two platinum bulbs about 0.75 inch in diameter in dilute sulphuric acid.

Owing to the large resistance (1000 Ohms) used in R and R', the actual potential is uncertain in this experiment, because the conduction across the fluid reduces it.

1. Potential in terms of a cell of Daniell's battery.	2. Duration of electrifi- cation in seconds.	3. Swing of reflecting galvanometer by the discharge of the bulbs on raising the key.				4. Current after magnet came to rest.	5. Mean minus the remaining current.	6. Mean divided by potential and 100 to give relative capacity for various potentials.	7. Approximate capa- city in micro- farads.
0.02	10	2½	2½	2½	2½	½	2	1	348
"	20	2½	2½	2½	2½	"	4	1	"
0.04	10	4½	4½	4½	4½	"	6	1	"
0.06	10	6½	6½	6½	6½	"	10½	1.05	365
0.1	10	11½	11½	11½		1	16½	1.09	379
"	20	11	12	11½		"	22½	1.12	390
0.16	10	18	17½	18		1½	55½	1.39	484
"	20	18	18	18		"	104½	1.74	606
0.2	10	24	24	24		1½	159	1.99	693
"	20	24	24	24		2½	226	2.26	786
"	30	24	24	24		"	303	2.53	880
0.4	10	59	58	58		2½	426	3.04	1057
"	20	58	57	58		2½	562	3.5	1218
"	30	58	58	57		"			
0.6	10	105	105½	104		"			
"	20	105	104	105		"			
"	30	104	105	103		"			
0.8	10	164	163	162	162	3			
"	20	162	162	161		"			
"	30	162	162	161		"			
1.0	10	230	235	230	230	5			
"	20	232	231	230	231	"			
"	30	231	231			"			
*1.2	30	318	320	314		14			
*1.4	30	440	446	451		23			
*1.6	30	about 603				30, 41, 52			

* Last three readings doubtful, the current remaining after the discharge being considerable. The true reading would be greater than indicated.

Condenser of 311 microfarads.

0.02	...	1½	1½	1½	1½	1.03	311
0.04	...	3	3	3	3	"	"
0.06	...	4½	4½	4½	4½	"	"
0.08	...	6	6	6	6	"	"
0.10	...	7½	7½	7½	7½	"	"
0.2	...	14½	14½	14½	14½	1	"
0.4	...	29	29	28½	29	"	"
0.6	...	43	43½	44	43½	"	"
0.8	...	58	58	58	58	"	"
1.0	...	72½	73	73	73	"	"
1.6	...	116	116	116	116	"	"
2.0	...	143	143	143	143	"	"

The condenser of 311 microfarads capacity consisted of 24,300 square feet of metal surface insulated by thin paper and paraffin wax.

TABLE II.

Two platinum plates in acid and water, each exposing 1 square inch surface. The resistance of $R + R' = 100$ Ohms in this Table; by experiment the potential of the two cells was found to be reduced 8 per cent., and was therefore very nearly 200 lbs. instead of two cells Daniell's.

Approximate potential in volts.	Time of electrification.	Throw of image by discharge of plates.				Current remaining after discharge.	Mean value of the current.	Ratio of capacity with different potentials.	Value in microfarads.
	seconds.								
0.2	10	19	20	19		1	18	1	175
"	20	18	19	19		"			
0.4	10	45	46	46		3	43	1.3	210
"	20	46	46			"			
0.8	10	175	170	170	165	11	169	2.3	385
1.0	10	230	228	226		18	210	2.33	408
*1.2	10	310	308	311		22	288	2.67	467
*1.4	10	373	380	382		30	350	2.77	484
*1.6	10	460	460	467	475	33	428	3.10	542
Condenser of 311 microfarads.									
0.2	.	32	32	32		0	32	1	311
0.4	..	63	64	63½		"	63½	"	"
0.8		127	127			"	127	"	"
1.0		159	159			"	159	"	"
1.2	..	188	187	189		"	188	"	"
1.4	.	220	220	221		"	220	"	"
1.6	...	252	254	252	254	"	253	"	"
1.8	...	284	283	284		"	284	"	"
2.0	..	316	317	317		"	317	"	"

* These readings are uncertain, being obliged to guess how much current remained after the image had swung out and back, its momentum lasting longer than with smaller deflections; the true reading would therefore be greater than those observed.

January 19, 1871.

General Sir EDWARD SABINE, K.C.B., President, in the Chair.

Prof. Alfred Newton was admitted into the Society.

The following communications were read:—

- I. "On the Structure and Development of the Skull of the Common Frog (*Rana temporaria*). By W. KITCHEN PARKER, F.R.S. Received October 10, 1870.

(Abstract.)

At the close of my last paper "On the Skull of the Common Fowl," I spoke of bringing before the Royal Society another, treating of that of

the osseous fish. I was working at the early conditions of the salmon's skull at the time.

I was, however, led to devote my attention to another and more instructive type early in the following year ; for it was then (January 1869) that Professor Huxley was engaged in preparing his very important paper "On the Representation of the Malleus and Incus in the other Vertebrata" (see Zool. Proc. May 27, 1869).

In repeating some of his observations for my own instruction, it occurred to me to renew some researches I had been making from time to time on the frog and toad. The results were so interesting to us both, that it was agreed for me to work exhaustively at the development of the frog's skull before finishing the paper on that of the salmon. On this account Professor Huxley mentions in his paper (*op. cit.* p. 406) that he leaves the Amphibia out of his demonstration, and that they are to be worked out by me. The amount of metamorphosis demonstrable in the chick whilst enclosed in the egg suggested a much more definite series of changes in a low, slow-growing Amphibian type. I think that this has been fully borne out by what is shown in the present paper.

The first of the ten stages into which I have artificially divided my subject is the unhatched embryo, whilst its head and tail project only moderately beyond the yelk-mass. Another stage is obtained by taking young tadpoles on about the third day after they have escaped from their glairy envelope ; a few days elapse between the second and third stages, but a much longer time between the third and fourth, for the fourth stage is the perfect tadpole, before the limbs appear and whilst it is essentially a fish with mixed *Chimæroid* and *Myxinoid* characters. Then the metamorphosing tadpole is followed until it is a complete and nimble frog, two stages of which are examined ; and then old individuals are worked out, which give the culminating characters of the highest type of Amphibian.

The early stages were worked out principally from specimens hardened in a solution of chromic acid ; and the rich umber-brown colour of these preparations made them especially fit for examination by reflected light.

Without going further into detail as to the mode of working my subject out, and without any lengthened account of the results obtained, I may state that the following conclusions have been arrived at ; namely, that the skull of the adult is highly compound, being composed of :—

1st. Its own proper membranous sac ;

2nd. Of a posterior part which is a continuation, in an unsegmented form, of the vertebral column ;

3rd. Of laminae which grow upwards from the first pair of facial arches, and which enclose the fore part of the membranous sac, just as the "investing mass" of the cranial part of the notochord invests the hinder part.

4th. The ear-sacs and the olfactory labyrinth become inextricably combined with the outer case of the brain. And

5th. The subcutaneous tissue of the scalp becomes ossified in certain

definite patches; these are the cranial roof-bones. Around the mouth there are cartilages like those of the Lamprey and the Chimæra; but these yield in interest to the proper facial bars, which are as follows, namely:—

First pair, the “trabeculæ.”

Second pair, the mandibular arch.

Third pair, the hyoid arch.

And fourth to seventh pairs: these are the branchials.

These are all originally separate pairs of cartilaginous rods; and from these are developed all the complex structures of the mouth, palate, face, and throat. The pterygo-palatine arcade is merely a secondary connecting bar developed, after some time, between the first and second arches.

Meckel's cartilage arises as a segmentary bud from the lower part of the second, and the “stylo-cerato-hyal,” as a similar secondary segment, from the third arch.

By far the greater part of the cranium (its anterior two-thirds) is developed by out-growing laminæ from the trabeculæ, which after a time become fused with the posterior or vertebral part of the skull.

When the tadpole is becoming a frog, the hyoid arch undergoes a truly wonderful amount of metamorphosis.

The upper part, answering to the hyomandibular of the fish (not to the whole of it, but to its upper half), becomes the “incus,” and a detached segment becomes the “orbiculare,” which wedges itself between the incus and the “stapes.” The stapes is a “bung” cut out of the “ear-sac.” The stylo-cerato-hyal is set free, rises higher and higher, and then articulates with the “opisthotic” region of the ear-sac; in the toad it coalesces therewith, as in the mammal. The lower part of the hyomandibular coalesces with the back of the pair of the mandibular arch; and the “symplectic” of the osseous fish appears whilst the tadpole is acquiring its limbs and its lungs, and then melts back again into the arch in front; it is represented, however, in the bull-frog, but not in the common species, by a distinct bone.

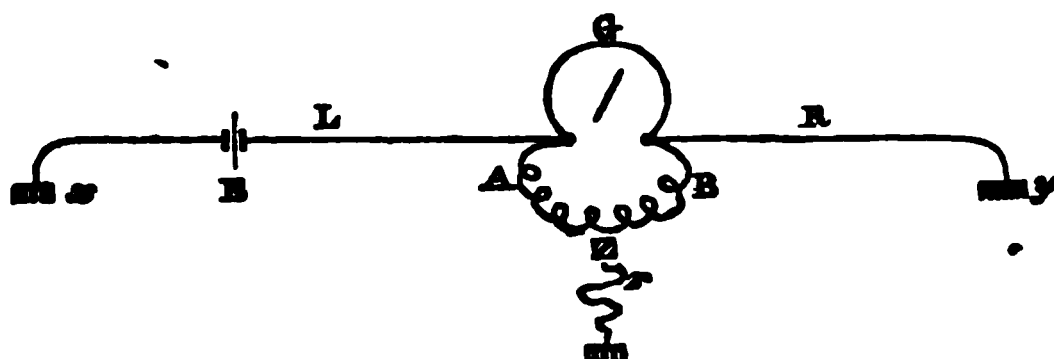
This very rough and imperfect abstract must serve at present to indicate what has been seen and worked out in this most instructive vertebrate.

II. “Method of measuring the Resistance of a Conductor or of a Battery, or of a Telegraph-Line influenced by unknown Earth-currents, from a single Deflection of a Galvanometer of unknown Resistance.” By HENRY MANCE, Superintendent Mekran Coast and Persian Gulf Telegraph Department, Kurrachee. Communicated by Sir WM. THOMSON. Received January 12, 1871.

The resistance of each part of a circuit, such as that shown in fig. 1, being known, the influence exercised by the shunt A B, as well as the

total resistance of the whole between x and y , can be easily ascertained by simple and well-known formulæ.

Fig. 1.



But let a leakage r , which we will suppose gives perfect earth, be applied at some point in the shunt $A B$, the deflection previously produced on G by a current arising in L will probably be considerably changed. I say probably, because by sliding the leakage r along the whole length of the shunt, we shall at last find a point Z at which the needle will return to its original deflection; the position of Z being ascertained, any resistance varying from infinity to "dead earth" may be applied without causing any change in the deflection of the needle.

It is evident that, although the total resistance of the circuit between x and y has been lessened by the insertion of the leakage, a proportionately larger amount of current is diverted from the galvanometer by that part of the shunt between L and the leakage at Z .

Presuming the electromotive E in L to remain constant, and taking $r=0$, we have the intensity of the current passing through G represented by the equation

$$\frac{E}{\left\{ L + \frac{G \cdot (A+B)}{G+(A+B)} + R \right\} \cdot \left\{ \frac{G+(A+B)}{A+B} \right\}};$$

but after r is connected, the equation becomes

$$\frac{E}{\left\{ L + \frac{\left(G + \frac{RB}{R+B} \right) A}{A + G + \frac{RB}{R+B}} \right\} \cdot \frac{G + \frac{RB}{R+B} + A}{A}}.$$

As the condition that the galvanometer deflection remains unchanged, the first of these equations must be equal to the second, from which we obtain the formula

$$L = R \cdot \frac{A}{B},$$

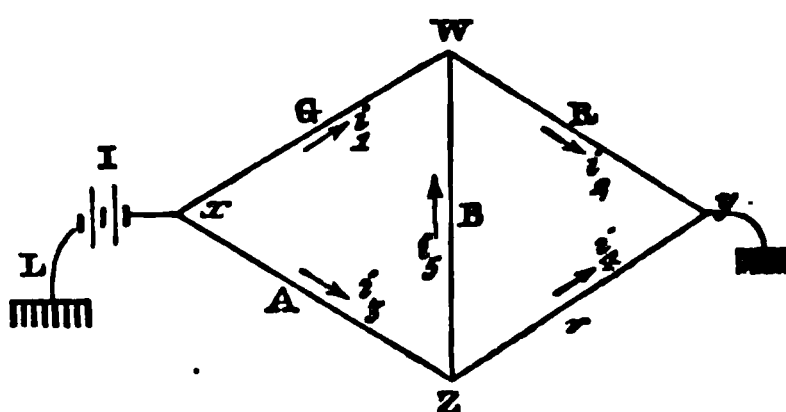
the resistance G being immaterial. It will therefore be seen that R always bears the same proportion to L that B does to A , the latter branches bearing some analogy to the proportion-coils of a Wheatstone testing bridge.

Under certain circumstances a test might be taken without any battery

at all. In a submerged cable there is frequently sufficient earth-current to supply the electromotive force in the branch L ; if not, a small battery can be inserted to maintain a steady current, and the internal resistance of the cells afterwards deducted. The polarization-current from a leakage of low resistance in a cable would enable us to find the resistance from either side through the fault without the application of a battery. And, lastly, this method may be used to ascertain the internal resistance of a battery.

The above method occurred to me about two years since during some experiments made to determine the resistance of the bridge-circuit and the exact proportion of current traversing each branch of the Wheatstone balance when the potentials at W and Z are unequal.

Fig. 2.



If I equals the intensity of the current at x or y , and i_1, i_2, i_3, i_4, i_5 the intensities in the sections G, R, A, r, B , then

$$\frac{G \cdot (B + R + r) + BR}{A \cdot (B + R + r) + Br} + 1 = \frac{I}{i_1} \dots \dots \dots (1)$$

$$\frac{A \cdot (B + R + r) + Br}{G \cdot (B + R + r) + BR} + 1 = \frac{I}{i_3} \dots \dots \dots (2)$$

$$\frac{R \cdot (A + B + G) + BG}{r \cdot (A + B + G) + AB} + 1 = \frac{I}{i_2} \dots \dots \dots (3)$$

$$\frac{r \cdot (A + B + G) + BA}{R \cdot (A + B + G) + BG} + 1 = \frac{I}{i_4} \dots \dots \dots (4)$$

$$\frac{B \cdot (R + r) + (B + R + r) \cdot (A + G)}{Gr - AR} = \frac{I}{i_5} \dots \dots \dots (5)$$

Or if the current in the branch B passes from W to Z ,
 $AR - Gr$

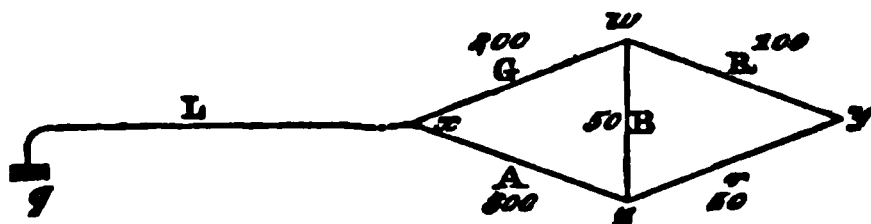
should be substituted for the denominator of the last equation.

Equations (1), (2), (3), and (4) give the shunt-coefficient of the respective branches A, G, r, R ; thus if G were a galvanometer, the strength of the deflection recorded multiplied by equation (1) would give the value of intensity I .

If, then, we consider G a galvanometer and the resistance r a leakage applied at Z , we have a similar diagram to that given in fig. 1 ; and the first of the five equations given above will enable us to determine the shunt-coefficient for the part A which lies between L and the leakage at Z .

Now this, together with the plan of testing described in the first paragraph, suggests an easy method for ascertaining by calculation the combined resistance of any system of derived circuits connected in the form of the Wheatstone's parallelogram; thus if I wish to know the resistance offered to the passage of a current between x and y in fig. 3, I can find it

Fig. 3.



in the following manner.

First assume the existence at x of a sixth branch bearing (in resistance) the same proportion to R that A does to B ; that is to say, the supposititious branch

$$L = R \cdot \frac{A}{B}.$$

Now disconnect r from the point Z , and we have again a diagram similar to that in fig. 1; and as we have provided that $\frac{A}{B} = \frac{L}{R}$, the connexion or disconnexion of r at the point Z will make no difference whatever in the quantity of current passing from L into the branch G . I may therefore assume that, although the total resistance of the circuit between q and y has been decreased, the branch A has at the same time been able to divert a proportionately greater amount of current from the side G , in which the intensity remains unaltered.

If, then,

R_1 equals the resistance between q and y when the branch r is disconnected,

S_1 the shunt-coefficient of $A B$ which forms a shunt in the absence of r ,

R_2 the resistance between q and y after r is connected at Z ,

S_2 the shunt-coefficient for the part A ascertained by equation (1),

we have

$$R_1 \times S_1 = R_2 \times S_2,$$

$$R_2 = \frac{R_1 S_1}{S_2};$$

and R_2 minus the supposititious branch $\left(\frac{RA}{B}\right)$ will give the required combined resistance of the circuit between x and y .

Let R_3 be the combined resistance. Commencing with the equation

$$\frac{\left\{ \frac{RA}{B} + \frac{G \cdot (A+B)}{G+(A+B)} + R \right\} \times \frac{A+B+G}{A+B}}{\frac{G \cdot (B+R+r) + BR}{A \cdot (B+R+r) + Br} + 1} - \frac{RA}{B} = R_3,$$

we obtain

$$R_s = \frac{\frac{R \cdot (A + B + G)}{B} + G}{\frac{G \cdot (B + R + r) + BR}{A \cdot (B + R + r) + Br} + 1} - \frac{RA}{B}.$$

If the potential at Z equalled that at W, the formula

$$R_s = \frac{(G + R) \cdot (A + R)}{G + R + A + r}$$

would of course be sufficient.

III. "Measurement of the Internal Resistance of a Multiple Battery by adjusting the Galvanometer to Zero." By HENRY MANCE. Communicated by Sir WM. THOMSON, LL.D., F.R.S. Received January 12, 1871.

The following method of taking the internal resistance of a battery will be found to give excellent results when several cells are to be tested.

Take one element from the rest of the cells and arrange the circuit as in the annexed figure. Connect the poles of the battery under observation by a shunt S, and adjust the resistance of the latter till zero is obtained on the galvanometer.

Let E be the number of cells tested,

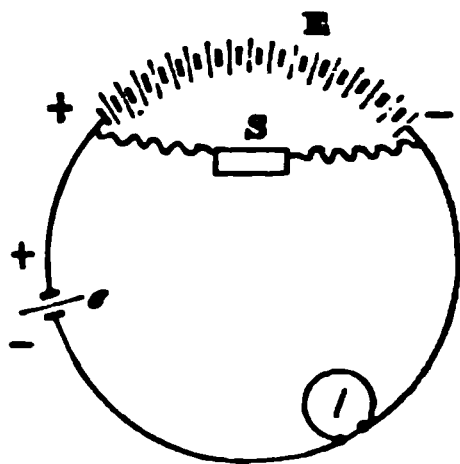
e number of cells opposed,

S = resistance of shunt,

R = internal resistance of E.

Then

$$R = S \frac{E}{e} - S.$$



In practice I usually returned the detached cell to the battery when $S \times E$ gave the internal resistance of the whole within a fraction of a unit.

It is assumed that the electromotive force in e equals that of the whole battery multiplied by $\frac{e}{E}$; the chance of error on account of this not being exactly the case would be lessened by detaching a larger number of cells than one when the internal resistance of the remaining portion would be given by the first formula.

IV. "Modification of Wheatstone's Bridge to find the Resistance of a Galvanometer-Coil from a single deflection of its own needle."

By Prof. Sir WILLIAM THOMSON, F.R.S. Received January 19, 1871.

In any useful arrangement in which a galvanometer or electrometer and a galvanic element or battery are connected, through whatever trains or network of conductors, let the galvanometer and battery be interchanged. Another arrangement is obtained which will probably be useful for a very different, although reciprocally related object. Hence, as soon as I learned from Mr. Mance his admirable method of measuring the internal resistance of a galvanic element (that described in the first of his two preceding papers), it occurred to me that the reciprocal arrangement would afford a means of finding the resistance of a galvanometer-coil, from a single deflection of its own needle, by a galvanic element of unknown resistance. The resulting method proves to be of such extreme simplicity that it would be incredible that it had not occurred to any one before, were it not that I fail to find any trace of it published in books or papers; and that personal inquiries of the best informed electricians of this country have shown that, in this country at least, it is a novelty. It consists simply in making the galvanometer-coil one of the four conductors of a Wheatstone's bridge, and adjusting, as usual, to get the zero of current when the bridge contact is made, with only this difference, that the test of the zero is not by a galvanometer in the bridge showing no deflection, but by the galvanometer itself, the resistance of whose coil is to be measured, showing an unchanged deflection. Neither diagram nor further explanation is necessary to make this understood to any one who knows Wheatstone's bridge.

V. "On a Constant Form of Daniell's Battery." By Prof. Sir WILLIAM THOMSON, F.R.S. Received January 19, 1871.

Graham's discovery of the extreme slowness with which one liquid diffuses into another, and Fick's mathematical theory of diffusion, cannot fail to suggest that diffusion alone, without intervention of a porous cell or membrane, might be advantageously used for keeping the two liquids of a Daniell's battery separate. Hitherto, however, no galvanic element without some form of porous cell, membrane, or other porous solid for separator, has been found satisfactory in practice.

The first idea of dispensing with a porous cell, and keeping the two liquids separate by gravity, is due to Mr. C. F. Varley, who proposed to put the copper-plate in the bottom of a jar, resting on it a saturated solution of sulphate of copper, resting on this a less dense solution of sulphate of zinc, and immersed in the sulphate of zinc the metal zinc-plate fixed

near the top of the jar. But he tells me that batteries on this plan, called "gravity-batteries," were carefully tried in the late Electric and International Telegraph Company's establishments, and found wanting in economy. The waste of zinc and of sulphate of copper was found to be more in them than in the ordinary porous-cell batteries. Daniell's batteries without porous cells have also been tried in France, and found unsatisfactory on account of the too free access of sulphate of copper to the zinc, which they permit. Still, Graham's and Fick's measurements leave no room to doubt but that the access of sulphate of copper to the zinc would be much less rapid if by true diffusion alone, than it cannot but be in any form of porous-cell battery with vertical plates of copper and zinc opposed to one another, as are the ordinary telegraphic Daniell's batteries which Mr. Varley finds superior to his own "gravity-battery." The comparative failure of the latter, therefore, must have arisen from mixing by currents of the liquids. All that seems necessary, therefore, to make the gravity-battery much superior instead of somewhat inferior to the porous-cell battery, is to secure that the lower part of the liquid shall always remain denser than the upper part. In seeking how to realize this condition, it first occurred to me to take advantage of the fact that saturated solution of sulphate of zinc is much denser than saturated solution of sulphate of copper. It seems* that, at 15° temperature, saturated aqueous solution of sulphate of copper is of 1.186 sp. gr., and contains in every 100 parts of water 33.1 parts of the crystalline salt; and that at 15° the saturated solution of sulphate of zinc is of sp. gr. 1.44, and contains in every 100 parts of water 140.5 parts of sulphate of zinc, both results being from Michel and Krafft's experiments†. Hence I made an element with the zinc below; next it saturated solution of sulphate of zinc, gradually diminishing to half strength through a few centimetres upwards; saturated sulphate of copper resting on this; and the copper-plate fixed above in the sulphate-of-copper solution. In the beginning, and for some time after, it is clear that the sulphate of copper can have no access to the zinc otherwise than by true diffusion. I have found this anticipation thoroughly realized in trials continued for several weeks; but the ultimate fate of such a battery is that the sulphate of zinc must penetrate through the whole liquid, and then it will be impossible to keep sulphate of copper separate in the upper part, because saturated solution of sulphate of zinc certainly becomes denser on the introduction of sulphate of copper to it. To escape this chaotic termination I have introduced a siphon of glass with a piece of cotton-wick along its length inside it, so placed as to draw off liquor very gradually from a level somewhat nearer the copper than the zinc; and a glass funnel, also provided with a core of cotton-wick, by which water semisaturated with sulphate of zinc may be continually introduced at a somewhat lower level. A galvanic

* Storer's Dictionary of Solubilities of Chemical Substances. Cambridge, Massachusetts: Sever and Francis, 1864.

† Ann. Ch. et Phys. (3) vol. xli. pp. 478, 482: 1854.

element thus arranged will undoubtedly continue remarkably constant for many months; but it has one defect, which prevents me from expecting permanence for years. The zinc being below, must sooner or later, according to the less or greater vertical dimensions of the cell, become covered with precipitated copper from the sulphate of copper which finds its way (however slowly) to the zinc. On the other hand, if the zinc be above, the greater part of the deposited copper falls off incoherently from the zinc through the liquid to the copper below, where it does no mischief, provided always that the zinc be not amalgamated,—a most important condition for permanent batteries, pointed out to me many years ago by Mr. Varley. Placing the zinc above has also the great practical advantage that, even when after a very long time it becomes so much coated with metallic copper as to seriously injure the electrical effect, it may be removed, cleaned, and replaced without otherwise disturbing the cell; whereas if the zinc be below, it cannot be cleaned without emptying the cell and mixing the solutions, which will entail a renewal of fresh separate solutions in setting up the cell again. I have therefore planned the following form of element, which cannot but last until the zinc is eaten away so much as to fall to pieces, and which must, I think, as long as it lasts, have a very satisfactory degree of constancy.

The cell is of glass, in order that the condition of the solutions and metals which it contains may be easily seen at any time. It is simply a cylindrical or rectangular jar with a flat bottom. It need not be more than 10 centimetres deep; but it may be much deeper, with advantage in respect to permanence and ease of management, when very small internal resistance is not desired. A disk of thin sheet copper is laid at its bottom. A properly shaped mass of zinc is supported in the upper part of the jar. A glass tube (which for brevity will be called the charging-tube) of a centimetre or more internal diameter, ending in a wide saucer or funnel above, passes through the centre of the zinc, and is supported so as to rest with its lower open end about a centimetre above the copper. A glass siphon with cotton-wick core is placed so as to draw liquid gradually from a level about a centimetre and a half above the copper. The jar is then filled with semisaturated sulphate-of-zinc solution. A copper wire or stout ribbon of copper coated with india-rubber or gutta-percha passes vertically down through the liquid to the copper-plate below, to which it is riveted or soldered to secure metallic communication. Another suitable electrode is kept in metallic communication with the zinc above. To put the cell in action, fragments of sulphate of copper, small enough to fall down through the charging-tube, are placed in the funnel above. In the course of a very short time the whole liquid below the lower end of the charging-tube becomes saturated with sulphate of copper, and the cell is ready for use. It may be kept always ready by occasionally (once a week for instance) pouring in enough of fresh water, or of water quarter saturated with sulphate of zinc at the top of the cell,

to replace the liquid drawn off by the siphon from near the bottom. A cover may be advantageously added above, to prevent evaporation. When the cell is much used, so that zinc enough is dissolved, the liquid added above may be pure water; or if large internal resistance is not objected to, the liquid added may be pure water, whether the cell has been much used or not; but after any interval, during which the battery has not been much in use, the liquid added ought to be quarter saturated, or even stronger solution of sulphate of zinc, when it is desired to keep down the internal resistance. It is probable that one or more specific-gravity beads kept constantly floating between top and bottom of the heterogeneous fluid will be found a useful adjunct, to guide in judging whether to fill up with pure water or with sulphate-of-zinc solution. They may be kept in a place convenient for observation by caging them in a vertical glass tube perforated sufficiently to secure equal density in the horizontal layers of liquid, to be tested by the floaters.

An extemporized cell on this plan was exhibited to the Royal Society, and its resistance (measured as an illustration of Mance's method, described in the first of his two previous communications) was found to be $\cdot 29$ of an Ohm (that is to say, 290,000,000 centimetres per second). The copper and zinc plates of this cell, being circular, were about 30 centimetres in diameter, and the distance between them was about 7.5 centimetres. A Grove's cell, of such dimensions that forty in series would give an excellent electric light, was also measured for resistance, and found to be $\cdot 19$ of an Ohm. Its intensity was found to be 1.8 times that of the new cell, which is the usual ratio of Grove's to Daniell's; hence seventy-two of the new cells would have the intensity of forty of Grove's. But the resistance of the seventy-two in series would be 209 Ohms, as against 76 Ohms of the forty Grove's; hence, to get as powerful an electric light, threefold surface, or else diminished resistance by diminished distance of the plates, would be required. How much the resistance may be diminished by diminishing the distance rather than increasing the surface, it is impossible to deduce from experiments hitherto made.

Two or three cells, such as the one shown to the Royal Society, will be amply sufficient to drive a large ordinary turret-clock without a weight; and the expense of maintaining them will be very small in comparison with that of winding the clock. The prime cost of the heavy wheel-work will be avoided by the introduction of a comparatively inexpensive electromagnetic engine. For electric bells, and all telegraphic testing and signaling on shore, the new form of battery will probably be found easier of management, less expensive, and more trustworthy than any of the forms of battery hitherto used. For use at sea, it is probable that the sawdust Daniell's, first introduced on board the 'Agamemnon' in 1858, and ever since that time very much used both at sea and on shore, will still probably be found the most convenient form; but the new form is certainly better for all ordinary shore uses.

The accompanying drawing represents a design suitable for the electric light, or other purposes, for which an interior resistance not exceeding $\frac{1}{10}$ of an Ohm is desired. The zinc is in the form of a grating, to prevent



the lodgment of bubbles of hydrogen gas, which I find constantly, but very slowly, gathering upon the sines of the cells I have tried, although the



solutions used have no free acid, unless such as may come from the ordinary commercial sulphate of copper and commercial sulphate-of-zinc crystals which were used.

POSTSCRIPT.

Received February 2, 1871.

The principle which I have adopted for keeping the sulphate of copper from the zinc is to allow it no access to the zinc except by true diffusion. This principle would be violated if the whole mass of the liquid contiguous to the zinc is moved toward the zinc. Such a motion actually takes place in the second form of element (that which is represented in the drawing, and which is undoubtedly the better form of the two) every time crystals of sulphate of copper are dropped into the charging-tube. As the crystals dissolve, the liquid again sinks, but not through the whole range through which it rose when the crystals were immersed. It sinks further as the sulphate of copper is electrically precipitated on the copper plate below in course of working the battery. Neglecting the volume of the metallic copper, we may say, with little error, that the whole residual rise is that corresponding to the volume of water of crystallization of the crystals which

have been introduced and used. It becomes, therefore, a question whether it may not become a valuable economy to use anhydrous sulphate of copper instead of the crystals; but at present we are practically confined to the "blue vitriol" crystals of commerce, and therefore the quantity of water added at the top of the cell from time to time must be, on the whole, at least equal to the quantity of water of crystallization introduced below by the crystals. Unless a cover is added to prevent evaporation, the quantity of water added above must exceed the water of crystallization introduced below by at least enough to supply what has evaporated. There ought to be a further excess, because a downward movement of the liquid from the zinc to the level from which the siphon draws is very desirable to retard the diffusion of sulphate of copper upwards to the zinc. Lastly, this downward movement is also of great value to carry away the sulphate of zinc as it is generated in the use of the battery. The quantity of water added above ought to be regulated so as to keep the liquid in contact with the zinc a little less than half saturated with sulphate of zinc, as it seems, from the observations of various experimenters, that the resistance of water semisaturated with sulphate of zinc is considerably less than that of a saturated solution. A still more serious inconvenience than a somewhat increased resistance has been pointed out to me by Mr. Varley as a consequence of allowing sulphate of zinc to accumulate in the battery. Sulphate of zinc crystallizes over the lip of the jar, and forms pendants like icicles outside, which act as capillary siphons, and carry off liquid. Mr. Varley tells me that this curious phenomenon is not unfrequently observed in telegraph-batteries, and sometimes goes so far as to empty a cell and throw it altogether out of action. Even without this extreme result, the crystallization of zinc about the mouth of the jar is very inconvenient and deleterious. It is of course altogether avoided by the plan I now propose.

In conclusion, then, the siphon-extractor must be arranged to carry off all the water of crystallization of the sulphate of copper decomposed in the use of the cell, and enough of water besides to carry away as much sulphate of zinc as is formed in the use of the battery. Probably the most convenient mode of working the system in practice will be to use a glass capillary siphon, drawing quickly enough to carry off in a few hours as much water as is poured in each time at the top; and to place, as shown in the drawing, the discharging end of the siphon so as to limit the discharge to a level somewhat above the upper level of the zinc grating. It will no doubt be found convenient in practice to add measured amounts of sulphate of copper by the charging-tube each time, and at the same time to pour in a measured amount of water, with or without a small quantity of sulphate of zinc in solution.

As 100 parts by weight of sulphate of copper crystals contain, as nearly as may be, 36 parts of water, it may probably answer very well to put in, for every kilogramme of sulphate of copper, half a kilogramme of water. Experience (with the aid of specific-gravity beads) will no doubt render it very

easy, by a perfectly methodical action involving very little labour, to keep the battery in good and constant action, according to the circumstances of each case.

When, as in laboratory work, or in arrangements for lecture-illustrations, there may be long intervals of time during which the battery is not used, it will be convenient to cease adding sulphate of copper when there is no immediate prospect of action being required, and to cease pouring in water when little or no colour of sulphate of copper is seen in the solution below. The battery is then in a state in which it may be left untouched for months or years. All that will be necessary to set it in action again will be to fill it up with water to replace what has evaporated in the interval, and stir the liquid in the upper part of the jar slightly, until the upper specific-gravity bead is floated to near the top by sulphate of zinc, and then to place a measured amount of sulphate of copper in the funnel at the top of the charging-tube.

VI. "On the Determination of a Ship's Place from Observations of Altitude." By Sir WILLIAM THOMSON. Received Feb. 6, 1871.

The ingenious and excellent idea of calculating the longitude from two different assumed latitudes with one altitude, marking off on a chart the points thus found, drawing a line through them, and concluding that the ship was somewhere on that line at the time of the observation, is due to Captain T. H. Sumner*. It is now well known to practical navigators. It is described in good books on navigation, as, for instance, Raper's (§§ 1009-1014). Were it not for the additional trouble of calculating a second triangle, this method ought to be universally used, instead of the ordinary practice of calculating a single position, with the most probable latitude taken as if it were the true latitude. I believe, however, that even when in a channel, or off a coast trending north-east and south-west, or north-west and south-east, where Sumner's method is obviously of great practical value, some navigators do not take advantage of it; although no doubt the most skilful use it habitually in all circumstances in which it is advantageous. I learned it first in 1858, from Captain Moriarty, R.N., on board H.M.S. 'Agamemnon.' He used it regularly in the Atlantic Telegraph expeditions of that year and of 1865 and 1866, not merely at the more critical times, but in connexion with each day's sights. Instead of solving two triangles, as directed by Captain Sumner, the same result may be obviously obtained by

* 'A new and accurate method of finding a Ship's Position at Sea,' by Capt. T. H. Sumner. Boston, 1843. "In 1843, Commander Sullivan, R.N., not having heard of this work, found the line of equal altitude on entering the River Plate; and identifying the ship's place on it in 12 fathoms by means of the chart, shaped his course up the river. The idea may thus have suggested itself to others; but the credit of having reduced it to a method and made it public belongs to Capt. Sumner." (Raper's Navigation, edition 1857.)

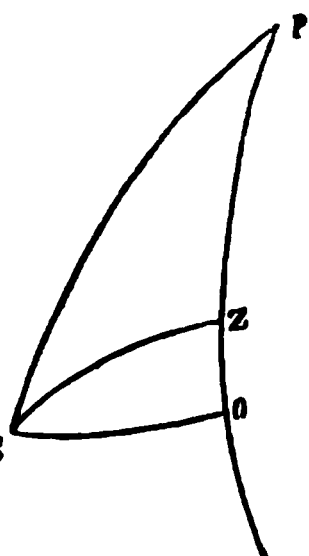
finding a second angle (Z) of the one triangle (PZS) ordinarily solved (P being the earth's pole, Z the ship's zenith, and S the sun or star). The angle ordinarily calculated is P , the hour-angle. By calculating Z , the sun's azimuth also, from the same triangle, the locus on which the ship must be is of course found by drawing on the chart, through the point which would be the ship's place were the assumed latitude exactly correct, a line inclined to the east and west at an angle equal to Z . But, as Captain Moriarty pointed out to me, the calculation of the second angle would involve about as much work as solving for P a second triangle with a slightly different latitude; and Capt. Sumner's own method has practical advantages in affording a check on the accuracy of the calculation by repetition with varied data.

A little experience at sea suggests that it would be very desirable to dispense with the morning and evening spherical triangles altogether, and to abolish calculation as far as possible in the ordinary day's work. When we consider the thousands of triangles daily calculated among all the ships at sea, we might be led for a moment to imagine that every one has been already solved, and that each new calculation is merely a repetition of one already made; but this would be a prodigious error; for nothing short of accuracy to the nearest minute in the use of the data would thoroughly suffice for practical purposes. Now, there are 5400 minutes in 90° , and therefore there are 5400^2 or 157,464,000,000 triangles to be solved each for a single angle. This, at 1000 fresh triangles per day, would occupy above 400,000 years. Even with an artifice, such as that to be described below, for utilizing solutions of triangles with their sides integral numbers of degrees, the number to be solved (being 90^2 or 729,000) would be too great, and the tabulation of the solutions would be too complicated (on account of the trouble of entering for the three sides) to be convenient for practice; and Tables of this kind which have been actually calculated and published (as, for instance, Lynn's *Horary Tables* *) have not come into general use.

It has occurred to me, however, that by dividing the problem into the solution of two right-angled triangles, it may be practically worked out so as to give the ship's place as accurately as it can be deduced from the observations, without any calculation at all, by aid of a table of the solution of the 8100 right-angled spherical triangles of which the legs are integral numbers of degrees.

Let O be the point in which the arc of a great circle s less than 90° through S , perpendicular to PZ , meets PZ or PZ produced.

If the data were SP , PZ , and the hour-angle P , the solution of the right-angled triangle SPO would give PO and SO . Subtracting PZ



* *Horary Tables for finding the time by inspection &c.*, by Thomas Lynn, late Commander in the sea-service of the East-India Company. London, 1827, 4to.



b.	$\alpha=51^\circ$		$\alpha=52^\circ$		$\alpha=53^\circ$		$\alpha=54^\circ$		$\alpha=55^\circ$		$\alpha=56^\circ$		$\alpha=57^\circ$		$\alpha=58^\circ$		b.
	co-hyp.	A.	co-hyp.	A.	co-hyp.	A.	co-hyp.	A.	co-hyp.	A.	co-hyp.	A.	co-hyp.	A.	co-hyp.	A.	
1	35 0	89 11	38 0	89 13	37 0	89 15	36 0	89 16	35 0	89 18	34 0	89 20	33 0	89 21	32 0	89 23	1
2	38 58	88 23	37 58	88 26	36 58	88 30	35 59	88 33	34 59	88 36	33 59	88 39	32 59	88 42	31 59	88 45	2
3	38 56	87 34	37 56	87 40	36 56	87 44	35 57	87 49	34 57	87 54	33 57	87 59	32 57	88 3	31 57	88 8	3
4	38 53	86 46	37 53	86 53	36 54	86 59	35 54	87 6	34 54	87 12	33 54	87 18	32 53	87 24	31 53	87 30	4
5	38 49	85 58	37 50	86 6	36 50	86 15	35 50	86 23	34 51	86 30	33 51	86 38	32 52	86 46	31 48	86 53	5
6	38 45	85 10	37 45	85 20	36 46	85 30	35 46	85 39	34 47	85 49	33 47	85 54	32 48	86 7	31 44	86 16	6
7	38 39	84 22	37 40	84 34	36 41	84 45	35 41	84 56	34 42	85 7	33 42	85 18	32 43	85 29	31 39	85 39	7
8	38 33	83 34	37 34	83 48	36 35	84 1	35 36	84 14	34 37	84 26	33 37	84 38	32 38	84 50	31 34	85 2	8
9	38 26	82 47	37 27	83 2	36 28	83 17	35 29	83 31	34 30	83 45	33 32	83 59	32 33	84 12	31 34	84 25	9
10	38 18	82 0	37 19	82 17	36 21	82 33	35 22	82 49	34 24	83 4	33 25	83 19	32 26	83 34	31 27	83 48	10
11	38 9	81 13	37 11	81 31	36 13	81 49	35 14	82 6	34 16	82 23	33 18	82 40	32 19	82 56	31 21	83 12	11
12	38 0	80 27	37 2	80 46	36 4	81 6	35 6	81 25	34 8	81 43	33 10	82 1	32 11	82 19	31 13	82 36	12
13	37 49	79 41	36 52	80 2	35 54	80 23	34 56	80 43	33 59	81 3	33 1	81 22	32 2	81 41	31 5	82 0	13
14	37 38	78 55	36 41	79 18	35 44	79 40	34 46	80 2	33 49	80 23	32 53	80 44	31 54	81 4	30 57	81 24	14
15	37 26	78 10	36 29	78 34	35 33	78 58	34 36	79 21	33 39	79 44	32 42	80 6	31 44	80 28	30 47	80 49	15
16	37 13	77 25	36 17	77 51	35 21	78 16	34 24	78 41	33 28	79 5	32 31	79 28	31 34	79 51	30 37	80 14	16
17	37 0	76 41	36 4	77 8	35 8	77 35	34 12	78 0	33 16	78 26	32 20	78 51	31 23	79 15	30 27	79 39	17
18	36 46	75 57	35 50	76 26	34 55	76 54	33 59	77 21	33 4	77 47	32 8	78 14	31 12	78 39	30 16	79 4	18
19	36 31	75 14	35 36	75 44	34 41	76 13	33 46	76 42	32 51	77 9	31 55	77 37	31 0	78 4	30 4	78 30	19
20	36 15	74 31	35 21	75 2	34 26	75 33	33 32	76 3	32 37	76 32	31 42	77 1	30 47	77 29	29 52	77 56	20
21	35 50	73 49	35 5	74 9	34 11	74 52	33 17	75 24	32 23	75 55	31 28	76 25	30 34	76 54	29 39	77 23	21
22	35 34	73 6	34 49	74 16	33 45	74 16	32 43	75 10	31 51	75 30	30 56	76 12	29 57	76 19	29 24	76 51	22
23	35 18	72 24	34 33	73 34	33 29	73 34	32 29	74 28	31 35	74 59	30 40	76 0	29 41	75 37	29 8	76 34	23
24	35 2	71 42	34 17	72 52	33 13	72 52	32 14	74 46	31 19	74 46	30 24	75 16	29 25	75 21	29 0	76 18	24
25	34 46	71 0	33 51	72 10	32 57	72 10	31 58	74 34	30 53	74 34	29 58	75 4	29 9	75 7	28 44	76 2	25
26	34 30	70 19	33 35	71 29	32 41	71 29	31 42	73 53	30 37	73 53	29 42	75 1	29 0	75 1	28 28	75 46	26
27	34 14	69 38	33 19	70 48	32 25	70 48	31 26	73 12	30 21	73 12	29 26	74 20	28 46	74 20	28 12	75 30	27
28	33 58	68 57	33 3	69 67	32 9	69 67	31 10	72 31	30 5	72 31	29 10	73 39	28 30	73 39	27 56	75 14	28
29	33 42	68 16	32 47	68 86	31 43	68 86	30 44	71 50	29 49	71 50	28 53	72 58	28 14	72 58	27 40	75 0	29
30	33 26	67 35	32 31	67 105	31 27	67 105	30 28	71 9	29 33	71 9	28 37	72 17	28 0	72 17	27 24	74 44	30
31	33 10	66 54	32 15	66 29	31 11	66 29	30 12	70 28	29 17	70 28	28 21	71 36	27 46	71 36	27 8	74 28	31
32	32 54	66 13	31 59	65 48	30 55	65 48	29 56	69 47	28 51	69 47	28 5	70 55	27 30	70 55	26 52	74 12	32
33	32 38	65 32	31 43	64 67	30 39	64 67	29 40	68 66	28 35	68 66	28 0	69 74	27 14	69 74	26 36	73 56	33
34	32 22	64 51	31 27	63 86	30 23	63 86	29 24	67 85	28 19	67 85	27 44	68 92	26 58	68 92	26 20	73 40	34
35	32 6	64 10	31 11	63 105	30 7	63 105	29 8	66 104	28 7	66 104	27 28	67 116	26 42	67 116	26 4	73 24	35
36	31 50	63 29	30 55	62 29	29 51	62 29	28 52	65 28	28 0	65 28	27 12	66 36	26 26	66 36	25 48	73 8	36
37	31 34	62 48	30 39	61 48	29 35	61 48	28 36	64 47	27 46	64 47	26 56	65 54	26 10	65 54	25 32	72 52	37
38	31 18	61 67	30 23	60 67	29 19	60 67	28 20	63 66	27 30	63 66	26 40	64 74	25 54	64 74	25 16	72 36	38
39	31 2	60 86	30 7	59 86	29 3	59 86	28 4	62 85	27 14	62 85	26 24	63 92	25 38	63 92	24 52	72 20	39
40	30 46	60 5	29 51	59 105	28 58	59 105	27 48	61 104	26 58	61 104	25 8	62 116	25 22	62 116	24 36	72 4	40
41	30 30	59 24	29 35	58 29	28 42	58 29	27 32	60 103	26 42	60 103	25 52	61 110	25 6	61 110	24 20	71 48	41
42	30 14	58 43	29 19	57 48	28 26	57 48	27 16	59 102	26 26	59 102	25 36	60 106	25 40	60 106	24 4	71 32	42
43	29 58	57 62	29 3	56 67	28 10	56 67	27 0	58 101	26 10	58 101	25 20	59 110	25 24	59 110	23 48	71 16	43
44	29 42	56 81	28 47	55 86	27 54	55 86	26 44	57 100	25 54	57 100	25 4	58 104	25 8	58 104	23 32	71 0	44
45	29 26	55 100	28 31	54 105	27 38	54 105	26 28	56 99	25 38	56 99	25 14	57 108	25 18	57 108	23 16	70 44	45
46	29 10	54 19	28 15	53 19	27 22	53 19	26 12	55 98	25 22	55 98	25 0	56 102	25 2	56 102	23 0	70 28	46
47	28 54	53 38	27 59	52 38	27 6	52 38	25 56	54 97	25 6	54 97	24 46	55 106	24 46	55 106	22 44	70 12	47
48	28 38	52 57	27 43	51 57	26 50	51 57	25 40	53 96	25 0	53 96	24 30	54 100	24 30	54 100	22 28	69 56	48
49	28 22	51 76	27 27	50 76	26 34	50 76	25 24	52 95	24 44	52 95	24 14	53 94	24 14	53 94	22 12	69 40	49
50	28 6	50 95	27 11	49 95	26 18	49 95	25 8	51 94	24 28	51 94	24 0	52 98	24 0	52 98	21 56	69 24	50
51	27 50	50 14	26 55	48 14	26 2	48 14	25 52	50 93	24 12	50 93	23 42	51 92	23 42	51 92	21 40	69 8	51
52	27 34	49 33	26 39	47 33	25 46	47 33	25 36	49 92	24 0	49 92	23 26	50 96	23 26	50 96	21 24	68 44	52
53	27 18	48 52	26 23	46 52	25 30	46 52	25 20	48 91	23 44	48 91	23 10	49 90	23 10	49 90	21 8	68 28	53
54	27 2	48 11	26 7	45 11	25 14	45 11	25 4	47 90	23 28	47 90	22 54	48 84	22 54	48 84	20 52	68 12	54
55	26 46	47 30	25 51	44 30	24 58	44 30	24 48	46 89	23 12	46 89	22 38	47 83	22 38	47 83	20 36	67 56	55
56	26 30	46 49	25 35	43 49	24 42	43 49	24 32	45 88	22 56	45 88	22 22	46 82	22 22	46 82	20 20	67 40	56
57	26 14	45 68	25 19	42 68	24 26	42 68	24 16	44 87	22 40	44 87	22 6	45 81	22 6	45 81	20 4	67 24	57
58	25 58	44 87	25 3	41 87	24 10	41 87	23 50	43 86	22 24	43 86	21 40	44 80	21 40	44 80	19 52	67 8	58
59	25 42	44 6	24 47	40 6	23 54	40 6	23 34	42 85	22 8	42 85	21 24	43 74	21 24	43 74	19 36	66 52	59
60	25 26	43 25	24 31	39 25	23 38	39 25	23 18	41 84	21 42	41 84	21 8	42 68	21 8	42 68	19 20	66 36	60
61	25 10	42 44	24 15	38 44	23 22	38 44	23 2	40 83	21 26	40 83	20 52	41 57	20 52	41 57	19 4	66 20	61
62	24 54	41 63	23 59	37 63	23 6	37 63	22 46	39 82	21 10	39 82	20 36	40 46	20 36	40 46	18 48	66 4	62
63	24 38	40 82	23 43	36 82	22 50	36 82	22 30	38 81	20 54	38 81	20 20	39 40	20 20	39 40	18 32	65 48	63
64	24 22	40 1	23 27	35 1	22 44	35 1	22 14	37 80	20 38	37 80	20 4	38 34	20 4	38 34	18 16	65 32	64
65	24 6	39 20	23 11	34 20	22 28	34 20	21 58	36 79	20 22	36 79	19 48	37 28	19 48	37 28	18 0	65 16	65
66	23 50	38 39	22 55	33 39	22 12	33 39	21 42	35 78	20 6	35 78	19 32	36 22	19 32	36 22	17 44	65 0	66
67	23 34	37 58	22 39	32 58	21 56	32 58	21 26	34 77	19 50	34 77	19 16	35 16	19 16	35 16	17 28	64 44	67
68	23 18	37 17	22 23	31 17													

from PO , we have ZO ; and this, with SO in the triangle SZO , gives the zenith distance, SZ , and the azimuth, SZO , of the body observed.

Suppose, now, that the solution of the right-angled spherical triangle SPO for PO and SO to the nearest integral numbers of degrees could suffice. Further, suppose PZ to be the integral number of degrees closest to the estimated co-latitude, then ZO will be also an integral number of degrees. Thus the two right-angled spherical triangles SPO and SZO have each arcs of integral numbers of degrees for legs. Now I find that the two steps which I have just indicated can be so managed as to give, with all attainable accuracy, the whole information deducible from them regarding the ship's place. Thus the necessity for calculating the solutions of spherical triangles in the ordinary day's work at sea is altogether done away with, provided a convenient Table of the solutions of the 8100 triangles is available. I have accordingly, with the cooperation of Mr. E. Roberts, of the 'Nautical Almanac' Office, put the calculation in hand; and I hope soon to be able to publish a Table of solutions of right-angled spherical triangles, showing co-hypotenuse* and one angle, to the nearest minute, for every pair of values of the legs from 0° to 90° . The rule to be presently given for using the Tables will be readily understood when it is considered that the data for the two triangles are their co-hypotenuses, the difference between a leg of one and a leg of the other, and the condition that the other leg is common to the two triangles. The Table is arranged with all the 90 values for one leg (b) in a vertical column, at the head of which is written the value of the other leg (a). Although this value is really not wanted for the particular nautical problem in question, there are other applications of the Table for which it may be useful. On the same level with the value of b , in the column corresponding to a , the Table shows the value of the co-hypotenuse and of the angle A opposite to the leg a . I take first the case in which latitude and declination are of the same name, the latitude is greater than the declination, and the azimuth (reckoned from south or north, according as the sun crosses the meridian to the south or north of the zenith of the ship's place) is less than 90° . The hypotenuses, legs, and angles P and Z of the two right-angled triangles of the preceding diagram are each of them positive and less than 90° , and the two co-hypotenuses are the sun's declination and altitude respectively. We have then the following rule:—

(1) Estimate the latitude to the nearest integral number of degrees by dead reckoning.

(2) Look from one vertical column to another, until one is found in which co-hypotenuses approximately agreeing with the declination and altitude are found opposite to values of b which differ by the complement of the assumed latitude.

(3) The exact values of the co-hypotenuse and the angle A corresponding

* It is more convenient that the complements of the hypotenuses should be shown than the hypotenuses, as the trouble of taking the complements of the declination and the observed altitude is so saved.

to these values of b are to be taken as approximate declination, hour-angle, altitude, and azimuth.

(4) Either in the same or in a contiguous vertical column find similarly another set of four approximate values, the two sets being such that one of the declinations is a little less and the other a little greater than the true declination.

(5) On the assumed parallel of latitude mark off the points for which the actual hour-angles at the time of observation were exactly equal to the approximate hour-angles thus taken from the Table. With these points as centres, and with radii equal (miles for minutes) to the differences of the approximate altitude from the observed altitude, describe circles. By aid of a parallel ruler and protractor *, draw tangents to these circles, inclined to the parallel of latitude, at angles equal to the approximate azimuths taken from the Table. These angles, if taken on the side of the parallel away from the sun, must be measured from the easterly direction, or the westerly direction, according as the observation was made before or after noon. The tangent must be taken on the side of the circle towards the sun, or from the sun, according as the observed altitude was greater or less than the approximate altitude taken from the Tables in each case. The two tangents thus drawn will be found very nearly parallel. Draw a line dividing the space between them into parts proportional to the differences of the true declination, from the two approximate values taken from the Tables. *The ship's place at the time of the observation was somewhere on the line thus found.*

To facilitate the execution of clause (2) of the rule, a narrow slip of card should be prepared with numbers 0 to 90 printed or written upon it at equal intervals, in a vertical column, equal to the intervals in the vertical column of the Table, 0 being at the top and 90 at the bottom of the column as in the Table. Place number 90 of the card abreast of a value of co-hypotenuse in the Table approximately equal to the declination, and look for the other co-hypotenuse abreast of the number on the card equal to the assumed latitude. Shift the card from column to column according to this condition until the co-hypotenuse abreast of the number on the card equal to the assumed latitude is found to agree approximately enough with the observed altitude.

When the declination and latitude are of contrary names and the azimuth less than 90° , or when they are of the same names, but the declination greater than the latitude, the sum, instead of the difference, of the legs b of the two triangles will be equal to the complement of the assumed latitude; and clause (2) of the rule must be altered accordingly. The slip of card in this case cannot be used; but the following scarcely less easy process is to be practised. Put one point of a pair of compasses on a position in one of the vertical columns of the hypotenuse abreast of

* A circle divided to degrees, and having its centre at the centre of the chart, ought to be printed on every chart. This, rendering in all cases the use of a separate protractor unnecessary, would be useful for many purposes.

point of the column of values of b corresponding to half the complement of the assumed latitude; this point will be on a level with one of the numbers, or midway between that of two consecutive numbers, according as the assumed latitude is even or odd: then use the compasses to take pairs of co-hypotenuses equidistant in the vertical column from the fixed point of the compasses, and try from one column to another until the co-hypotenuses approximately agreeing with the observed altitude and the assumed declination are found. It is easy to modify the rule so as to suit the case in which the azimuth is an obtuse angle; but it is not worth while to do so at present, as such cases are rarely used in practice.

The following examples will sufficiently illustrate the method of using the tables:—

Example 1. On 1870, May 16, afternoon, at 5h. 42m. Greenwich apparent time, the Sun's altitude was observed to be $32^{\circ} 4'$: to find the ship's place, the assumed latitude being 54° North.

The Nautical Almanac gives at 1870, May 16, 5h. 42m. Greenwich apparent time, the Sun's apparent declination N. $19^{\circ} 10'$. On looking in the annexed Table (which is a portion of the solutions of the right-angled spherical triangles) under the heading $a = 56^{\circ}$, and opposite $b = 54^{\circ}$, the co-hypotenuse (representing the Sun's declination) is $19^{\circ} 11'$, and opposite $b = 18^{\circ}$ (differing from 54° by the complement of the assumed latitude), the co-hypotenuse (representing the Sun's altitude) is $31^{\circ} 55'$, which are sufficiently near the actual values; we therefore select the following sets of values from these columns as follows:—

		Co-hyp.	A.	
$a = 56^{\circ}$	1.	$b = 54^{\circ}$	$19^{\circ} 11'$	$61^{\circ} 23' =$ Sun's hour-angle.
		$b = 18^{\circ}$	$32^{\circ} 8'$	$78^{\circ} 14' =$ Sun's azimuth (S. towards W.).
	2.	$b = 55^{\circ}$	$18^{\circ} 42'$	$61^{\circ} 5' =$ Sun's hour-angle.
		$b = 19^{\circ}$	$31^{\circ} 55'$	$77^{\circ} 37' =$ Sun's azimuth (S. towards W.).

which we have the following:—

Greenwich apparent time (in arc)	$85^{\circ} 30'$	$85^{\circ} 30'$
Sun's hour-angle	(1) $61^{\circ} 23'$(2)	$61^{\circ} 5'$
	<hr/>	<hr/>
Diff. = Longitude	$24^{\circ} 7' \text{ W.}$	$24^{\circ} 25' \text{ W.}$
	<hr/>	<hr/>
Sun's altitude (observed)	$32^{\circ} 4'$	$32^{\circ} 4'$
Sun's altitudes (auxiliary)	(1) $32^{\circ} 8'$(2)	$31^{\circ} 55'$
	<hr/>	<hr/>
Diff. =	$- 4'$	$+ 9'$
	<hr/>	<hr/>
Sun's declination from N. A.	$19^{\circ} 10'$	$19^{\circ} 10'$
Sun's declinations (auxiliary)	(1) $19^{\circ} 11'$(2)	$18^{\circ} 42'$
	<hr/>	<hr/>
Diff =	$- 1'$	$+ 28'$
	<hr/>	<hr/>

This example is represented graphically in the first diagram annexed. The second set of values could have been selected equally well from the contiguous columns ($a=57^\circ$), which on trial will be found to give an almost identical result.

Again, (2), on 1870, May 16, afternoon, at 5h. 42m. Greenwich apparent time, the Sun's altitude was observed to be $30^\circ 30'$: to find the ship's place, the assumed latitude being 10° North.

The Sun's declination from N. A. is $N. 19^\circ 10'$, and the half complement of the assumed latitude 40° . By a few successive trials, $a=56^\circ$ will be found to contain values of co-hypotenuses approximately equal to the Sun's declination and altitude at the time, and which are equidistant from 40° ; we therefore select the following sets of values from this column as follows:—

		Co-hyp.	A.	
$a = 56^\circ$	1.	$b=54$	$19\ 11$	$61\ 23$ = Sun's hour-angle.
		$b=28$	$30\ 10$	$73\ 32$ = Sun's azimuth (N. towards W.).
	2.	$b=55$	$18\ 43$	$61\ 5$ = Sun's hour-angle.
		$b=27$	$29\ 53$	$72\ 58$ = Sun's azimuth (N. towards W.).

from which we have the following:—

Greenwich apparent time (in arc)	$85\ 30$	$85\ 30$
Sun's hour-angle	(1) $61\ 23$	(2) $61\ 5$
	<hr/>	<hr/>
Diff. = Longitude	$24\ 7\ W.$	$24\ 25\ W.$
	<hr/>	<hr/>
Sun's altitude (observed)	$30\ 30$	$30\ 30$
Sun's altitudes (auxiliary)	(1) $30\ 10$	(2) $29\ 53$
	<hr/>	<hr/>
Diff. =	$+ 20$	$+ 37$
	<hr/>	<hr/>
Sun's declination from N. A.	$19\ 10$	$19\ 10$
Sun's declinations (auxiliary)	(1) $19\ 11$	(2) $18\ 43$
	<hr/>	<hr/>
Diff. =	$- 1$	$+ 28$
	<hr/>	<hr/>

In this case the sun passes the meridian to the north of the ship's zenith, the azimuth, from the Tables being less than 90° , is measured from the north towards the west. In this case also the second set of values might have been taken from $a=57^\circ$, which will be found on trial to give a position nearly identical with the above.

This example is represented in the second diagram annexed.

Again, (3), on 1870, May 16, afternoon, at 5h. 42m. Greenwich apparent time, the Sun's altitude was observed to be $18^\circ 35'$: to find the ship's place, the assumed latitude being 20° South.

The Sun's declination from N. A. is $N. 19^\circ 10'$, and the half complement of the assumed latitude is 55° , to be used because the Sun's declination and the assumed latitude are of different names. Proceeding as in the previous

example, we find the column $a=56^{\circ}$ again to contain values of co-hypotenuses approximately equal to the given values ; and therefore have :—

	Co-hyp.	A.	
$a = 56^{\circ}$	1. $b=54$	$19^{\circ} 11'$	$61^{\circ} 23' = \text{Sun's hour-angle.}$
	$b=56$	$18 13$	$60 47 = \text{Sun's azimuth (N. towards W.).}$
	2. $b=55$	$18 42$	$61 5 = \text{Sun's hour-angle.}$
	$b=57$	$17 44$	$60 30 = \text{Sun's azimuth (N. towards W.).}$

which give

Greenwich apparent time (in arc)	$85^{\circ} 30'$	$85^{\circ} 30'$
Sun's hour-angle	(1) $61 23$(2)	$61 5$
	<hr/>	<hr/>
Diff. = Longitude	$24 7 \text{ W.}$	$24 25 \text{ W.}$
	<hr/>	<hr/>
Sun's altitude (observed)	$18^{\circ} 35'$	$18^{\circ} 35'$
Sun's altitudes (auxiliary)	(1) $18 13$(2)	$17 44$
	<hr/>	<hr/>
Diff. =	$+ 22$	$+ 51$
	<hr/>	<hr/>
Sun's declination from N. A.	$19^{\circ} 10'$	$19^{\circ} 10'$
Sun's declinations (auxiliary)	(1) $19 11$(2)	$18 42$
	<hr/>	<hr/>
Diff. =	$- 1$	$+ 28$
	<hr/>	<hr/>

This example is represented in the third diagram annexed.

January 26, 1871.

General Sir EDWARD SABINE, K.C.B., President, in the Chair.

The following communications were read :—

- I. “On the Mineral Constituents of Meteorites.” By NEVIL STORY MASKELYNE, M.A., F.R.S., Professor of Mineralogy, Oxford, and Keeper of the Mineral Department, British Museum. Received November 3, 1870.

(Abstract.)

In the memoir now offered to the Society the author gives the results of his investigation of the meteorites of Breitenbach and of Shalka. A preliminary notice of two of the minerals occurring in the former, which is of the Siderolite class, was read before the Society in March, 1869 (Proc. R. S. vol. xvii. p. 370).

After entering upon the probable history of the Breitenbach Siderolite, and endeavouring to identify it with certain other Siderolites that have been found, or have been recorded as found, in the region extending from

Meissen to Breitenbach, the author proceeds to describe the individual minerals which constitute the mass of the Siderolite.

These are: first, a bronzite with the formula $(\text{Mg}_{\frac{4}{3}} \text{Fe}_{\frac{1}{3}}) \text{Si O}_3$, orthorhombic in its crystalline form. The crystallography of the mineral was investigated by Dr. Viktor von Lang at the British Museum, and has been published in Pogg. Annalen, vol. cxxxix. p. 315. Secondly, a mineral composed of silica, having the specific gravity of quartz after fusion, and crystallized in the orthorhombic system.

Since his preliminary notice was published, the crystallography of this substance has been carefully studied by the author, and the details are given in the memoir.

The elements of the crystal are

$$a : b : c : = 1.7437 : 1 : 3.3120.$$

The angles are :—

$$\begin{aligned} 100 : 101 &= 27^\circ 46' \\ 100 : 110 &= 60^\circ 10' \\ 110 : 101 &= 63^\circ 19' \end{aligned}$$

The optic axes lie in a plane parallel to the plane 010; the first mean line being the normal to the plane 100.

They are widely separated, presenting in air an apparent angle of about 107° .

There can thus be no question that this mineral is orthorhombic; and if the tridymite of Vom Rath is, as that distinguished crystallographer asserts it to be, hexagonal in its symmetry, the mineral in the Breitenbach meteorite will be a trimorphic form of silica. Such a result obtained from the investigation of a meteorite is one of no small interest.

The nickeliferous iron, the chief constituent of the Siderolite, proved on analysis to be an alloy of the formula $\text{Fe}_{10} \text{Ni}$, and contained a trace of copper. In addition to the two siliceous minerals, the iron encloses occasional crystals of chromite in well-developed octahedra, an iron sulphide, probably troilite, and a small amount of Schreibersite.

The author then proceeds to detail the results obtained from the analysis of the Shalka meteorite. In 1860, Haidinger, in his paper on this meteorite (Sitzber. d. k. Akad. Wiss. Wien, vol. xli. p. 251), held the entire stone to be made up of a mineral, which he termed Piddingtonite, and which, according to Von Hawr's analysis, might be a compound of bisilicate and trisilicate of iron and magnesium. This latter acid silicate, however, which has long haunted the mineralogy of meteorites, no more forms a constituent of this meteorite than does the other acid silicate Shepardite, as Dr. Laurence Smith has shown, enter into the composition of the Bishopville meteorite.

The view held by Haidinger, that this meteorite, though apparently made up of two silicates, a grey and a mottled variety, was nevertheless

composed of a single mineral species varying in colour, is proved by the analytical results given in this memoir. It has been found to be a bronzite of the formula $(\text{Mg}_3 \text{ Fe}_1) \text{ Si O}_3$, and in association with it there occurs some chromite in distinct crystals.

Rammelsberg has also recently published the results of an examination of this meteorite (Pogg. Annalen, vol. cxlii. p. 275), and finds in it a bronzite associated with 12 per cent. of olivine. It is probable that the meteorite varies in its composition in different parts, and that Prof. Rammelsberg analyzed that portion where an olivinous ingredient was in appreciable preponderance.

The mottled kind was treated with hydrogen chloride in the cold, and subsequently with potash, and again with hydrogen sulphate and potash, but in each case it was noticed that the action of the acid was confined to that of a solvent. A little meteoric iron was dissolved, but no appreciable amount of olivine was found in the portion examined in the Laboratory at the British Museum.

II. "On the Organization of the *Calamites* of the Coal-measures."

By W. C. WILLIAMSON, F.R.S., Professor of Natural History in Owens College, Manchester. Received November 11, 1870.

(Abstract.)

Ever since M. Brongniart established his genus *Calamodendron*, there has prevailed widely a belief that two classes of objects had previously been included under the name of *Calamites*—the one a thin-walled Equisetaceous plant, the *Calamites* proper, and the other a hard-wooded Gymnospermous Exogen, known as *Calamodendron*. This distinction the author rejects as having no existence, the thick- and thin-walled examples having precisely the same typical structure. This consists of a central pith, surrounded by a woody zone, containing a circle of woody wedges, and enclosed within a bark of cellular parenchyma.

The Pith has been solid in the first instance, but very soon became fistular, except at the nodes, at each one of which a thin diaphragm of parenchyma extended right across the medullary cavity. Eventually the pith underwent a complete absorption, thus enlarging the fistular interior until it became coextensive with the inner surface of the ligneous zone.

The Woody Zone.—This commenced in very young states by the formation of a circle of canals stretching longitudinally from one node to the adjoining one. Externally to, but in contact with, these canals a few barred or reticulated vessels were found; successive additions to these were made in lines radiating from within outwards; hence each wedge consisted of a series of radiating laminæ, separated by medullary rays, having a peculiar mural structure. At their commencement these wedges were separated by wide cellular areas, running continuously from node to node;

as the woody tissues increased exogenously, these cellular tracts also extended outwards. Radial longitudinal sections exhibited in these the same nural tissue that occurs in the woody wedges. Hence the author gives to the former the name of primary medullary rays, and to the latter that of secondary ones. The structure of the medullary and ligneous zones is compared with that of the stem of a true Exogen of the first year, of which transitional form *Calamites* may be regarded as a permanent representative. Tangential sections of this woody zone exhibit parallel bands of alternating vascular and cellular tissue, running from node to node. At the latter points each vascular band dichotomizes, its divergent halves meeting corresponding ones from contiguous wedges, and each two unite to form one of the corresponding bands or wedges of the next adjoining internode.

The Bark, hitherto undescribed, consists of a thick layer of cellular parenchyma, undivided into separate laminæ, and not exhibiting any special differentiation of parts. This structure exhibits no signs of external ridges or furrows, being apparently smooth. The stem was enlarged at each node, but the swelling was less due to any increased thickness of the bark at these points, than to an expansion of the woody layer at these points, both externally and internally. This was the result of the intercalation of numerous short vessels, which arched across each node, their concavities being directed inwards, and which constituted the portion of the woody zone that encroached upon the constricted pith at these nodes. Several modifications of the above type have been met with, most of which may have had a specific value. In one form no canals exist at the inner angles of the woody wedges; in another, laminæ, like those of the woody wedges, are developed in the more external portions of the primary medullary rays, those occupying the centre of each ray being the most external and latest formed. The primary ray is thus transformed into a series of secondary ones.

In another type the vascular laminæ of each woody wedge are few in number, and the component vessels are the same; but the latter are remarkable for their large size. In a fourth variety, the exterior of the woody zone has been almost smooth, instead of exhibiting the usual ridges and furrows: this variety is also remarkable for the large size of its medullary cells, compared with that of the cells and vessels of the woody zone.

But the most curious modification is seen in a plant previously described by the author under the name of *Calamopitus*, in which round or oblong canals are given off from the medullary cavity, and pass horizontally through each primary medullary ray of the woody zone to the bark. These, being arranged in regular verticils below each node, are designated the infranodal canals. The verticils of small round or oblong scars, seen at one extremity of the internodes of some *Calamites*, are the results of this peculiar organization. In one species of this *Calamopitus*, instead of the longitudinal canals of the woody wedges terminating at the nodes, they

bifurcate, like the wedges with which they are associated, and are continuously prolonged from internode to internode.

The ordinary structureless fossils found in shales and sandstones receive a definite interpretation from the specimens described. The fistular medullary cavities due in the first instance, *not* to decay of the tissues, but to the rapid growth of the stem, became further enlarged by the entire absorption of the true pith, which commenced after the latter had fulfilled its purpose in the origination of the woody wedges. This process terminated at an undulating line of arrested absorption, the convexities of which projected outwards, opposite the primary medullary rays, and inwards, opposite the woody wedges; and the inorganic cast of the cavity thus formed by a physiological action constitutes the *Calamites* commonly seen in collections. Hence they are not, like the *Sternbergiæ*, casts of a cavity within a true pith, but their form represents that of the exterior of the medullary tissue. The ridges and furrows of these *internal* casts are not identical in position with the similar undulations of the *exterior* of the woody zone, but alternate with them; so that the ligneous cylinder projects both externally and internally where the woody wedges are located, and contracts, in like manner, at the intermediate points opposite to the primary medullary rays. The thin carbonaceous film which frequently invests these casts is the residue of the altered elements of the woody zone, and possibly also of the bark, which latter has been very liable to become detached from the former. The surface-markings of this carbonaceous film have usually no structural significance, being merely occasioned by the impression of the hardened casts which they invest.

Two kinds of branches are given off by *Calamites*,—the one subterranean, springing from peculiarly formed rhizomes, and the other aerial, attached to the upright unbranched stems. The former of these are of comparatively large size, the nodes from which they have been detached being marked by large concave lenticular scars as phragmata. These branches appear to have been given off from central rhizomes in accordance with a regular phyllotaxis, but which varied in different species. The aerial branches, on the other hand, were merely slender appendages to a virtually unbranched stem; they were arranged in verticils round the nodes, in variable numbers. Each branch sprang from the interior of one of the woody wedges, the two halves of which were forced asunder to admit the base of the appendage, and from which its constituent vessels were derived. The branch, deprived of its bark, never appears to have had a diameter equal that of two of the woody wedges, and the rarity of their occurrence attached to the stem seems to indicate that they were deciduous. The bark investing them is not yet known, and the exact nature of the foliage which they bore is also uncertain, owing to discordant testimony respecting it; but there appears no reason for doubting that some of the verticillate *Asterophyllites* or *Annulariæ* represent it, though there is uncertainty respecting the actual forms to be identified with *Calamites*. The roots

were given off from the lower part of each internode, but above the node, and were apparently epidermal.

There is also considerable doubt respecting the fructification of *Calamites*. Some of the *Volkmanniæ* have evidently belonged to this group; but only one example retaining its minute organization has yet been found in which the structure of the central axes corresponded with that of the *Calamites*. The relationship to *Calamites* of the fruits figured by Binney, under the name of *Calamodendron commune*, which are identical with the *Volkmannia binneyi* of Carruthers, is more than doubtful, because of the anomalous structure of their central axes.

After a careful comparison of the organization of *Calamites* with that of the recent Equisetaceæ, the author prefers constituting the former an independent order, distinct from, though allied to, the Equisetums, under the name of *Calamitaceæ*, and characterized by cryptogamic fructification and verticillate foliage, associated with an exogenous axis. The latter feature probably involved the existence of something resembling a cambium layer, furnishing the material for the new tissues.

It is further proposed to divide these plants into two generic groups, *Calamites* and *Calamopitus*; the former to comprehend those unprovided with infranodal canals, and the latter those which possess them. The existing specific distinctions appear to have little or no scientific value.

II. "On Approach caused by Vibration." A Letter from Prof. Sir W. THOMSON, LL.D., F.R.S., &c. to Prof. FREDERICK GUTHRIE, B.A. Communicated by Sir W. THOMSON. Received November 17, 1870.

DEAR SIR,—I have to-day received the 'Proceedings of the Royal Society' containing your paper "On Approach caused by Vibration," which I have read with great interest. The experiments you describe constitute very beautiful illustrations of the established theorem for fluid pressure in abstract hydro-kinetics, with which I have been much occupied in mathematical investigations connected with vortex-motion.

According to this theorem, the average pressure at any point of an incompressible frictionless fluid originally at rest, but set in motion and kept in motion by solids moving to and fro, or whirling round in any manner, through a finite space of it, is equal to a constant diminished by the product of the density into half the square of the velocity. This immediately explains the attractions demonstrated in your experiments; for in each case the average of square of velocity is greater on the side of the card nearest the tuning-fork than on the remote side. Hence obviously the card must be attracted by the fork as you have found it to be; but it is not so easy at first sight to perceive that the average of the square of the velocity must be greater on the surfaces of the tuning-fork next to the

card than on the remote portions of the vibrating surface. Your theoretical observation, however, that the attraction must be mutual, is beyond doubt valid, as we may convince ourselves by imagining the stand which bears the tuning-fork and the card to be perfectly free to move through the fluid. If the card were attracted towards the tuning-fork, and there were not an equal and opposite force on the remainder of the whole surface of the tuning-fork and support, the whole system would commence moving, and continue moving with an accelerated velocity in the direction of the force acting on the card—an impossible result. It might, indeed, be argued that this result is not impossible, as it might be said that the kinetic energy of the vibrations could gradually transform itself into kinetic energy of the solid mass moving through the fluid, and of the fluid escaping before and closing up behind the solid. But “common sense” almost suffices to put down such an argument, and elementary mathematical theory, especially the theory of momentum in hydro-kinetics explained in my article on “Vortex-motion,” negatives it.

The law of the attraction which you observed agrees perfectly with the law of magnetic attraction in a certain ideal case which may be fully specified by the application of a principle explained in a short article communicated to the Royal Society of Edinburgh in February last, as an abstract of an intended continuation of my paper on “Vortex-motion.” Thus, if we take as an ideal tuning-fork two globes or disks moving rapidly to and fro in the line joining their centres, the corresponding magnet will be a bar with poles of the same name as its two ends and a double opposite pole in its middle. Again, the analogue of your paper disk is an equal and similar diamagnetic of infinite diamagnetic inductive capacity. The mutual force between the magnet and the diamagnetic will be equal and opposite to the corresponding hydro-kinetic force at each instant. To apply the analogy, we must suppose the magnet to gradually vary from maximum magnetization to zero, then through an equal and opposite magnetization back through zero to the primitive magnetization, and so on periodically. The resultant of fluid pressure on the disk is not at each instant equal and opposite to the magnetic force at the corresponding instant, but the average resultant of the fluid pressure is equal to the average resultant of the magnetic force. Inasmuch as the force on the diamagnetic is generally repulsion from the magnet, however the magnet be held, and is unaltered in amount by the reversal of the magnetization, it follows that the average resultant of the fluid pressure is an attraction on the whole towards the tuning-fork into whatever position the tuning-fork be turned relatively to it.

Your seventh experiment* has interested me even more than any of the others. It illustrates the elementary law of pressure in hydro-kinetics, not by showing effects of fluid pressure on portions of a solid bounding

* *Experiment 7* in *Proceedings Roy. Soc.* vol. xix. p. 38, or *experiment 10*, *Phil. Mag.* Nov. 1870.—F. G.

surface, as all other illustrative experiments hitherto known to me have done, but by showing an effect of diminished fluid pressure throughout more rapidly moving portions of the finite mass of the fluid itself. This effect consists of a slight degree of expansion, depending on the air not being perfectly incompressible. The volume occupied by the more rapidly moving portions becoming slightly augmented, the remainder of the fluid would be condensed were the whole contained within an altogether fixed boundary. A moveable portion of this boundary (that is, the surface of the liquid in your tube) yields and shows to the eye the effect of the diminished pressure through the rapidly moving portions.

No branch of abstract dynamics has had a greater charm for the mathematical worker than hydro-kinetics, but it has not hitherto been made generally attractive by experimental illustrations. Such refined and beautiful experiments as those you describe, and especially your seventh, tend notably to give to this branch of dynamics quite a different place in popular estimation from that which it has held; but what is perhaps of even greater importance, they help greatly to clear the ideas of those who have made it a subject of mathematical study.

Yours truly,

WILLIAM THOMSON.

Presents received December 8, 1870.

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General Sir EDWARD SABINE, K.C.B., President, in the Chair.

The following communications were read :—

I. "On Linear Differential Equations.—No. IV." By W. H. L. RUSSELL, F.R.S. Received November 17, 1870.

I will now consider some interesting results in definite integrals obtained from the solution of linear differential equations.

Let us first consider the linear differential equation

$$(a_3 + b_3x) \frac{d^3y}{dx^3} + (a_2 + b_2x) \frac{d^2y}{dx^2} + (a_1 + b_1x) \frac{dy}{dx} + (a_0 + b_0x)y = 0,$$

where $b_3 = 1$.

Let

$$y = e^{\int x^s Z dx},$$

where the limits are to be determined. Substituting in the differential equation, and following the usual method, we find

$$Z = \frac{1}{b_3(iz)^3 + b_2(iz)^2 + b_1(iz) + b_0} e^{\int dz \frac{a_3(iz)^3 + a_2(iz)^2 + a_1(iz) + a_0}{b_3(iz)^3 + b_2(iz)^2 + b_1(iz) + b_0}}.$$

Now let

$$\frac{a_3(iz)^3 + a_2(iz)^2 + a_1(iz) + a_0}{b_3(iz)^3 + b_2(iz)^2 + b_1(iz) + b_0} = m - \frac{\lambda}{iz - \alpha} - \frac{\mu}{iz - \beta} - \frac{\nu}{iz - \gamma},$$

then we shall have

$$y = \int \frac{e^{ix(m+x)} \cdot dz}{(iz - \alpha)^{\lambda+1} (iz - \beta)^{\mu+1} (iz - \gamma)^{\nu+1}},$$

where the limits, determined by the usual method, are evidently $-\infty$ and $+\infty$.

We have, from the above equation,

$$a_3 = b_3 m,$$

$$a_2 = -b_3 \{\lambda + \mu + \nu + m(\alpha + \beta + \gamma)\},$$

$$a_1 = b_3 \{\lambda(\beta + \gamma) + \mu(\alpha + \gamma) + \nu(\alpha + \beta) + m(\alpha\beta + \alpha\gamma + \beta\gamma)\},$$

$$a_0 = -b_3 \{\lambda\beta\gamma + \mu\alpha\gamma + \nu\alpha\beta + m\alpha\beta\gamma\},$$

$$b_2 = -b_3(\alpha + \beta + \gamma),$$

$$b_1 = b_3(\alpha\beta + \alpha\gamma + \beta\gamma),$$

$$b_0 = -b_3\alpha\beta\gamma.$$

Hence the differential equation becomes

$$\begin{aligned} (m+x) \frac{d^3 y}{dx^3} - \{\lambda + \mu + \nu + (\alpha + \beta + \gamma)(m+x)\} \frac{d^2 y}{dx^2} \\ + \{\lambda(\beta + \gamma) + \mu(\alpha + \gamma) + \nu(\alpha + \beta) + (\alpha\beta + \beta\gamma + \alpha\gamma)(m+x)\} \frac{dy}{dx} \\ - \{\lambda\beta\gamma + \mu\alpha\gamma + \nu\alpha\beta + \alpha\beta\gamma(m+x)\} y = 0. \end{aligned}$$

The solution of this equation will be as follows :—

$$y = P e^{\alpha x} + Q e^{\beta x} + R e^{\gamma x},$$

where

$$P = A + Bx + Cx^2 + \dots + Hx^\lambda,$$

$$Q = A' + B'x + C'x^2 + \dots + H'x^\mu,$$

$$R = A'' + B''x + C''x^2 + \dots + H''x^\nu.$$

where $A, B, \dots, A', B', \dots, A'', B'' \dots$ are constants to be determined ;

$$\therefore \int_{-\infty}^{\infty} \frac{e^{iz(m+x)} d iz}{(iz - \alpha)^{\lambda+1} (iz - \beta)^{\mu+1} (iz - \gamma)^{\nu+1}} = P e^{\alpha x} + Q e^{\beta x} + R e^{\gamma x}.$$

Now let α be essentially negative, β and γ essentially positive. Then, since the integral cannot always go on increasing with (x) , we have $Q=0$, $R=0$;

$$\begin{aligned} \therefore \int_{-\infty}^{\infty} \frac{e^{iz(m+x)} d . iz}{(iz - \alpha)^{\lambda+1} (iz - \beta)^{\mu+1} (iz - \gamma)^{\nu+1}} \\ = P e^{\alpha x} = (A + Bx + Cx^2 + \dots + Hx^\lambda) e^{\alpha x} : \end{aligned}$$

the constants A, B, C, \dots , where one of them is known, may be determined by substitution in the differential equation

$$\begin{aligned} (m+x) \frac{d^3 y}{dx^3} - \{\lambda + \mu + \nu + (\beta + \gamma - 2\alpha)(m+x)\} \frac{d^2 y}{dx^2} \\ - \{\lambda(2\alpha - \beta - \gamma) + \mu(\alpha - \gamma) + \nu(\alpha - \beta) \\ - (\alpha^2 - \alpha\beta - \alpha\gamma + \beta\gamma)(m+x)\} \frac{dy}{dx} - \lambda(\alpha^2 - \alpha\beta - \alpha\gamma + \beta\gamma) y = 0. \end{aligned}$$

But they are better determined in the following way. We may evidently, without any loss of generality, put $m=0$. In this case we shall have

$$\int_{-\infty}^{\infty} \frac{e^{ixz} d . iz}{(iz - \alpha)^{\lambda+1} (iz - \beta)^{\mu+1} (iz - \gamma)^{\nu+1}} = (A + Bx + Cx^2 + \dots + Hx^\lambda) e^{\alpha x}.$$

If we put $x=0$ in this integral, we have

$$A = \int_{-\infty}^{\infty} \frac{d . iz}{(iz - \alpha)^{\lambda+1} (iz - \beta)^{\mu+1} (iz - \gamma)^{\nu+1}}.$$

Differentiating and putting $x=0$,

$$\int_{-\infty}^{\infty} \frac{iz \cdot diz}{(iz-\alpha)^{\lambda+1}(iz-\beta)^{\mu+1}(iz-\gamma)^{\nu+1}} = A\alpha + B;$$

$$\therefore B = \int_{-\infty}^{\infty} \frac{d \cdot iz}{(iz-\alpha)^{\lambda}(iz-\beta)^{\mu+1}(iz-\gamma)^{\nu+1}};$$

and so we may proceed. The integral is thus completely determined.

A similar process will give us the more general integral

$$\int_{-\infty}^{\infty} \frac{e^{izx} d \cdot iz}{(iz-\alpha)^{\lambda+1}(iz-\beta)^{\mu+1}(iz-\gamma)^{\nu+1} \dots (iz-\zeta)^{\omega+1}},$$

where α is essentially negative, and $\beta, \gamma \dots \zeta$ essentially positive*.

The same process will hold good when $\mu, \nu \dots$ are fractional; for it is manifest that the integral

$$\int_{-\infty}^{\infty} \frac{e^{izx} \cdot diz}{(iz-\alpha)^{\lambda+1}(iz-\beta)^{\mu+1}(iz-\gamma)^{\nu+1}} = P e^{\alpha x} + Q e^{\beta x} + R e^{\gamma x};$$

where, however,

$$Q = \dots \frac{A_{\nu}}{x^{\nu-\lambda}} + \frac{A_{\nu-1}}{x^{\nu-\lambda-1}} + \dots + A_2 x^{\lambda-2} + A_1 x^{\lambda-1} + A_0 x^{\lambda},$$

with a similar expression for R .

Now, as x increases without limit, the series

$$\frac{A_{\nu}}{x^{\nu-\lambda}} + \frac{A_{\nu+1}}{x^{\nu-\lambda+1}} + \dots$$

converges; for since x is arbitrary, the ratio of two consecutive terms, $\frac{A_{\nu-\lambda+1}}{A_{\nu}x}$, may be made as small as we please, however great ν becomes. Hence, as x increases without limit, the series $\frac{A_{\nu-\lambda}}{x^{\nu-\lambda}} + \dots$ approaches zero. Therefore, as we suppose β positive, $Q e^{\beta x}$ will increase without limit; and therefore as Q is supposed multiplied by an arbitrary constant, we must have $Q=0$. Hence the value of the integral where μ, ν, \dots are supposed fractional will also be $P e^{\alpha x}$, α being negative, and the constants in P determined as before.

Next, consider the integral

$$\int_{\alpha}^{\infty} \frac{e^{zx} (z-\alpha)^{\lambda-1} dz}{(z-\beta)^{\mu+1}(z-\gamma)^{\nu+1}},$$

where x is supposed negative, α and β essentially positive, β less than α ,

* It is proper here to remind the reader that the integral

$$\int_{-\infty}^{\infty} \frac{e^{i\beta x} dx}{(a+ix)^n} = \frac{2\pi\beta^{n-1} e^{-a\beta}}{\Gamma(n)}$$

does not hold good when a is negative.

γ essentially negative. Then the integral will satisfy the differential equation

$$\begin{aligned} x \frac{d^2 y}{dx^2} + \{\lambda - \mu - \nu - (\alpha + \beta + \gamma)x\} \frac{dy}{dx} \\ - \{\lambda(\beta + \gamma) - \mu(\alpha + \gamma) - \nu(\alpha + \beta) - (\alpha\beta + \beta\gamma + \alpha\gamma)x\} y \\ - \{\lambda\beta\gamma + \mu\alpha\gamma + \nu\alpha\beta + \alpha\beta\gamma x\} y = 0. \end{aligned}$$

We here suppose λ fractional, μ and ν positive and entire. The solution of the differential equation will be

$$y = P e^{\alpha x} + Q e^{\gamma x} + C \{P e^{\alpha x} \int X_1 e^{\frac{\alpha + \beta + \gamma}{2} x^2} dx + Q e^{\gamma x} \int X_2 e^{\frac{\alpha + \beta + \gamma}{2} x^2} dx\};$$

where P and Q are rational and entire functions of the orders μ and ν respectively, and X_1, X_2 functions of x , which it will be needless to write down. The integral will consequently be equal to this expression when the arbitrary constants are properly determined. The integral cannot go on increasing as x , supposed negative, increases. Hence $Q=0$ and $C=0$, and the integral becomes

$$\int_a^\infty \frac{e^{xz}(z-\alpha)^{\lambda-1} dz}{(z-\beta)^{\mu+1}(z-\gamma)^{\nu+1}} = (A + Bx + Cx^2 + \dots + Hx^\mu) e^{\alpha x},$$

where $A, B, \&c.$ are constants to be determined.

We find, as before,

$$A = \int_a^\infty \frac{(z-\alpha)^{\lambda-1} dz}{(z-\beta)^{\mu+1}(z-\gamma)^{\nu+1}},$$

$$B = \int_a^\infty \frac{(z-\alpha)^{\lambda-1} dz}{(z-\beta)^\mu (z-\gamma)^{\nu+1}},$$

$$\&c. = \&c.$$

And thus the integral is completely known.

I will now consider the differential equation

$$2n \frac{d^2 y}{dx^2} - x \frac{dy}{dx} + 2na^2 \frac{dy}{dx} - a^2 xy = 0.$$

It is easily seen that this equation is satisfied by the integral

$$y = \int_{-\infty}^\infty \frac{e^{-xu^2+au} du}{a^2+u^2}.$$

The solution of the differential equation is

$$y = C_1 \cos ax + C_2 \sin ax + C_3 \{ \cos ax \int e^{x^2} \sin ax dx - \sin ax \int e^{x^2} \cos ax dx \};$$

and these must be equivalent.

If we put $-u$ for u in this equation, we easily see that

$$\int_{-\infty}^\infty \frac{e^{-xu^2+au} du}{a^2+u^2} = \int_{-\infty}^\infty \frac{e^{-xu^2-au} du}{a^2+u^2};$$

hence the integral is unchanged if $-x$ is put for x , and therefore $C_1 = C_2 = 0$.

Hence

$$\int_{-\infty}^{\infty} \frac{e^{-\pi u^2 + xu} du}{a^2 + u^2} = c \cos ax;$$

\therefore putting $s=0$,

$$c = \int_{-\infty}^{\infty} \frac{e^{-\pi u^2} du}{a^2 + u^2};$$

$$\therefore \frac{dc}{da} - a^2 c = \int_{-\infty}^{\infty} e^{-\pi u^2} du = -\frac{\sqrt{\pi}}{\sqrt{a}}.$$

Integrating this equation, and choosing the arbitrary constant so that c may vanish when a is infinite,

$$c = \sqrt{\pi} e^{a^2} \int_a^{\infty} \frac{dn}{\sqrt{n}} e^{-a^2 n}.$$

Hence we shall have

$$\int_{-\infty}^{\infty} \frac{e^{-\pi u^2 + xu} du}{a^2 + u^2} = 2\sqrt{\pi} \cos ax e^{a^2} \int_{\sqrt{a}}^{\infty} d\mu e^{-a^2 \mu^2},$$

which last integral is exceedingly well known. It is manifest that we can reduce the integral $\int_{-\infty}^{\infty} \frac{e^{-\pi u^2 + xu} du}{a^{2r} + u^{2r}}$ to this by the method of partial fractions.

In concluding this paper, I desire to express the obligations I am under to Spitzer's 'Studien.'

II. "Measurements of Specific Inductive Capacity of Dielectrics, in the Physical Laboratory of the University of Glasgow." By JOHN C. GIBSON, M.A., and THOMAS BARCLAY, M.A. Communicated by Sir WILLIAM THOMSON. Received November 23, 1870.

(Abstract.)

This paper describes the instruments and processes employed in a series of experiments on the specific inductive capacity of paraffine, and the effect upon it of variations of temperature. The instruments described are the platymeter and the sliding condenser. The former of these was, in a rudimentary form, shown to the Mathematical and Physical Section of the British Association at its Glasgow Meeting in 1855, by W. Thomson. It consists of two equal and similar condensers employed for the comparison of electrostatic capacities. The sliding condenser is a condenser the capacity of which may be varied by known quantities by altering the effective area of the opposed surfaces. By means of these two instruments, along with the quadrant electrometer, the capacity of a condenser may be determined by equalizing the sliding condenser to it. The method of working, and the electrical actions upon which it depends, are described in detail. In

order to determine the capacity of the sliding condenser at the lower extremity of its range, a spherical condenser, so constructed that its capacity could be accurately determined in absolute measure, was employed. An apparent discrepancy in the results obtained, arising from an inequality in the condensers forming the platymeter, is then considered, and the method of deducing the true result investigated. A series of experiments is then described which gave 1.975 as the specific inductive capacity of paraffine, that of air being taken as unity, but failed to show whether this alters with variations of temperature. An improved form of condenser, composed of concentric brass cylinders with paraffine for the dielectric, and the results obtained from it, are then described. The measurements made at different temperatures show no variation of specific inductive capacity. In order to allow to the paraffine freedom of expansion with temperature, another form of condenser was employed, and the same results obtained. A series of experiments was then made on the expansion of paraffine with temperature, in order to estimate the effect of this upon the capacity of paraffine condensers. As a mean of the results, it was found that the linear expansion of paraffine at 9° C. is .000237 per degree. Some further measurements of the cylindrical condenser were made with the same result as before. Thus all the measurements of this condenser made at temperatures ranging from $-12^{\circ}.15$ to 24.35 C. show no variation of specific inductive capacity of paraffine with temperature. This was found to be 1.977, that of air being taken as unity.

In a note added to the paper a description is given of an improved form of sliding condenser.

III. "On the Uniform Flow of a Liquid." By HENRY MOSELEY, M.A., D.C.L., Canon of Bristol, F.R.S., and Corresponding Member of the Institute of France. Received December 1, 1870.

(Abstract.)

The resistance of every molecule of a liquid at rest which a solid (by moving through it) disturbs, contributes its share to the resistance which the solid experiences; so that the inertia of each molecule so disturbed and its shear must be taken into account in the aggregate, which represents the resistance the liquid offers to the motion of the solid. The motions communicated to the molecules of a liquid by a solid passing through it, and the resistances opposed to them, however, are so various, and so difficult to be represented mathematically, that in the present state of our knowledge of hydrodynamics the problem of the resistance of a liquid at rest to a solid in motion is perhaps to be considered insoluble. As it regards the opposite problem of the resistance of a solid at rest to a liquid in motion (as in the case of a liquid conveyed through a pipe), there are in like manner to be taken into account the disturbances created by that re-

sistance in what would otherwise have been the motion of each individual molecule of the liquid so disturbed.

This problem, however, is by no means so difficult as the other. There is, indeed, a case in which it admits of solution. It is that of a liquid flowing from a reservoir, in which its surface is kept always at the same level, through a circular pipe which is perfectly straight, and of the same diameter throughout, and of a uniform smoothness or roughness of internal surface, and always full of the liquid. The liquid would obviously in such a pipe arrange itself in infinitely thin cylindrical films coaxial with the pipe, all the molecules in the same film moving with the same velocity, but the molecules of different films with velocities varying from the axis of the pipe to its internal surface. The direction of the motions of the molecules of such a liquid being known, and all in the same film moving with the same velocity, which velocity is a function of the radius of the film, and the law of the resistance of each film to the slipping over it of the contiguous film being assumed to be known, as also the head of water, it is possible to express mathematically

(1st) the work done per unit of time by the force which gives motion to the liquid, and

(2nd) the work per unit of time of the several resistances to which the liquid in moving through the pipe is subjected, and

(3rd) the work accumulated per unit of time in the liquid which escapes—and thus to constitute an equation in which the dependent variables are the radius of any given film, and the velocity of that film. This equation being differentiated and the variables separated, and the resulting differential equation being integrated, there is obtained the formula

$$v_0 e^{-\frac{250r}{l}},$$

where v is the velocity of the film whose radius is r , and v_0 that of the central filament, and l the length of the pipe—the unit of length being one metre, and of time one second.

The method by which the author has arrived at this formula is substantially the same as that which he before used in a paper read before the Society on the "Mechanical Impossibility of the Descent of Glaciers by their weight only," and which he believes to be a method new to mechanical science. It was indeed to verify it in its application to liquids that he undertook the investigations which he now submits to the Society, which, however, he has pursued beyond their original object.

The recent experiments of MM. Darcy and Bazin* have supplied him with the means of this verification. These experiments, made with admirable skill and precision, on pipes upwards of 100 metres in length, and varying in diameter from 0^m·0122 to 0^m·5, under heads of water varying

* *Recherches Expérimentales relatives au mouvement de l'Eau dans les Tuyaux*, par H. Darcy: Paris, 1857. *Recherches Hydrauliques*, par MM. Darcy et Bazin: Paris, 1865.

in height from 0^m·027 to 30^m·714, include (together with numerous experiments on the quantity of water which flows per second from such pipes under different conditions) experiments on the velocities of the films of water at different distances from the axes of the pipes, made by means of an improved form and adaptation of the well-known tube of Pitot. These last-mentioned experiments afford the means of verifying the above-mentioned formulæ. With a view to this verification, the author has compared the formula with sixty of the experiments of M. Darcy, and stated the results in the first two Tables of his paper.

The discharge per 1" from a pipe of a given radius may be calculated from the above formula in terms of the velocity of the central filament. This calculation the author has made, and compared it with the results of eleven of M. Darcy's experiments.

Where in the formula which thus represents the discharge from a pipe of given radius, in terms of the velocity of the central filament, the radius is made infinite, an expression is obtained for the volume of liquid of a cylindrical form, but of infinite dimensions (laterally), which would be put in motion by a *single filament* of liquid which traversed its axis; and, conversely, it gives the volume of such a liquid in motion which would be held back by a filament of liquid kept at rest along its axis. Thus it explains the well-known retarding effect of filaments of grass and roots in retarding the velocities of streams.

It is the relation of the velocity of any film to that of the central filament which the author establishes in the above formula. To the complete solution of the problem it is necessary that he should further determine the actual velocity v_0 of the central filament. This is the object of the second part of his paper. This velocity being known, the actual discharge per 1" is known. The following is the formula finally arrived at:—

$$Q = C \cdot \left[e^{-\frac{250 R}{l}} - \frac{250 R}{l} - 1 \right] R \ h^{\frac{1}{2}} l^{\frac{3}{2}},$$

where

Q = discharge per 1" in cubic metres.

R = radius of pipe in metres.

l = length of ditto.

h = head of water.

C = a constant dependent on the state of the internal surface of the pipe.

The values of this constant C , as deduced from the experiments of M. Darcy are given,

1st, for new cast-iron pipes;

2nd, for the same covered with deposit;

3rd, for the above *cleaned*;

4th, for iron pipes coated internally with bitumen;

5th, for new leaden pipes;

6th, for glass pipes.

The author compares this formula with sixty-two of M. Darcy's experiments, and records the results of this comparison in the last three Tables of his paper.

The paper concludes with an investigation of the rise in the temperature of a liquid flowing through a pipe caused by the resistances which its coaxial films oppose to their motions on one another (or, as it is termed, their *frictions* on one another) and on the internal surface of the pipe. The pipe is in this investigation supposed to be of a perfectly non-conducting substance.

February 9, 1871.

General Sir EDWARD SABINE, K.C.B., President, in the Chair.

The following communications were read :—

- L. "On the Effect of Exercise upon the Bodily Temperature." By T. CLIFFORD ALLBUTT, M.A., M.D. Cantab., F.L.S., Member of the Alpine Club, &c. Communicated by Mr. BUSK. Received November 12, 1870.

(Abstract.)

The object of the author in carrying out the experiments recorded in the present paper was to inquire whether the regulating-power of the organism held good under great variations of muscular exertion. For this purpose he made frequent daily examinations of his own temperatures during a short walking tour in Switzerland, and found that the effect of continuous muscular exertion upon himself was to sharpen the curve of daily variation—the culmination being one or two tenths higher than usual, and the evening fall coming on more rapidly and somewhat earlier. Charts of the daily temperatures were handed in with the paper. The author made reference also to some observations of M. Lortet, which differed from his own. These observations, which did not come into Dr. Clifford Allbutt's hands until his own experiments were partially completed, were adduced by M. Lortet to prove that the human body was very defective in regulating-power under the demands of the combustion needed to supply the force expended in muscular exertion. Dr. Clifford Allbutt's results were very decidedly opposed to those of M. Lortet; for only on two occasions did he note the depressions of temperature which M. Lortet regards as constant. It would seem, however, that the body is more or less liable to such depressions when engaged in muscular exertion; but the cause of them is very obscure. Of the two low temperatures noted by the author, one occurred during a very easy ascent of lower slopes, and the second was observed during a descent. The author thinks that they may be due to some accidental deficiency in combustion, and inquires whether the capacity of the chest in different individuals may account for the varying in-

fluence of muscular effort upon them, and perhaps for the earlier or later sense of fatigue. The sphygmographic tracings added by M. Lortet to his temperature-charts seemed to show a great inadequacy of circulation.

II. "Observations of the Eclipse at Oxford, December 28, 1870."

By JOHN PHILLIPS, M.A., D.C.L., F.R.S., Professor of Geology in the University of Oxford. Received December 28, 1870.

At my observatory, situated about one third of a mile eastward from the great establishment founded in the name of Dr. Radcliffe, the beginning of the eclipse was obscured by a passing cloud: the end was recorded at $13^h 38' 38'' = 1^h 35' 0'' \cdot 9$ Oxford mean time.

The progress of the obscuration was observed at unclouded intervals in the first half of the period, continuously during a clear sky in the latter half. Finding it impracticable to observe and measure with ordinary micrometers in the early part of the phenomenon, I arranged to throw the image on a screen, and make my measures on it.

The driving-clock was affected by the extreme cold, so as to make it difficult to keep the sun's image to one place, and it was convenient for other reasons sometimes to shift the image vertically; the method which I employed, however, was independent of these displacements, and allowed of as many measurements of the cusps as might be desired.

It consisted simply in marking at any moment with pencil the situation of the cusps on the screen, and appending to each dot the time by the sidereal clock. Joining, after the eclipse, these dots by a straight line, and then transferring a parallel line of equal length to meet internally a circle representing the limb of the sun, of the same diameter as the solar image, the chord of the cusps at the given time was obtained, from which, by an easy method, the place of the moon's centre at the moment was derived. The apparent diameters of the sun and moon were obtained by measure of arcs on the screen.

The diagrams exhibit the whole process. In diagram fig. 1, four of the lines are drawn from the dots on the screen, A A, B B, C C, D D.

In fig. 2, equal and parallel lines are transferred to the solar circle, whose centre is S, so as to touch it internally at A' A', B' B', C' C', D' D'. For each of these lines the centre of the moon's place is marked (A'', B'', C'', D''); thus the line of the motion of the moon's centre is given, and the phase of greatest obscuration determined.

The line of motion of the moon's centre is obtained by ruling through the mid points between A'' and B'', B'' and C'', C'' and D''. The point on this line reached by the moon's centre at the moment of greatest obscuration is found by bisection in M. Drawing through M and S the bisecting line of greatest obscuration, the length of the sagitta ms is determined.

It is found by these observations that,

The sun's diameter being taken at	530
That of the moon is	540
The length of the sagitta <i>ms</i> is	100



Fig. 1. A A, B B, C C, D D, are lines joining the dots marking the cusps at four successive epochs during the eclipse.

2. A' A', B' B', C' C', D' D', are four lines equal and parallel to A A, B B, C C, D D in fig. 1, and made to touch internally the solar circle, whose centre is S; *ms* the sagitta at the moment of greatest obscuration. The moon's path passes below A'', above B'', and nearly coincides with C'' and D'', which are the places of the moon's centre for the cusps A', B', C', D'.

These numbers, according to the proportions given in the Nautical Almanac for the Radcliffe Observatory, would have been:—

Diameter of the sun	530·0
Diameter of the moon	538·8
Length of sagitta	99·1

The agreement is quite close enough to justify the belief that, in skilful hands, the method described may be in some cases very useful, it being by no means limited to eclipses. It is so simple that one can hardly suppose it not to have been already employed; but I have met with no notice of such being the case.

During the progress of the eclipse three thermometers were observed. One north of the house, screened from the sun and sky, sank from 26° at $11^{\text{h}} 40^{\text{m}}$ to $24^{\circ} 4'$ at $1^{\text{h}} 25^{\text{m}}$. One south of the house, indirectly influenced by solar radiation on neighbouring objects, rose from $26^{\circ} 75'$ at 11^{h} to $27^{\circ} 8'$ at noon, then sank to 26° at $12^{\text{h}} 40^{\text{m}}$, and rose to $27^{\circ} 3'$ at $1^{\text{h}} 35^{\text{m}}$. A third, on grass open to the sky, sank from $27^{\circ} 8'$ at $11^{\text{h}} 40^{\text{m}}$ to $23^{\circ} 5'$ at $1^{\text{h}} 25^{\text{m}}$, and remained at this point till $1^{\text{h}} 35^{\text{m}}$. Though on a limited scale, the influence both of solar and sky radiation is traceable in these observations.

III. "On the Problem of the In- and Circumscribed Triangle." By
A. CAYLEY, F.R.S. Received December 30, 1870.

(Abstract.)

The problem of the in and circumscribed triangle is a particular case of that of the in- and circumscribed polygon: the last-mentioned problem may be thus stated—to find a polygon such that the angles are situate in and the sides touch a given curve or curves. And we may in the first instance inquire as to the number of such polygons. In the case where the curves containing the angles and touched by the sides respectively are all of them distinct curves, the number of polygons is obtained very easily and has a simple expression: it is equal to twice the product of the *orders* of the curves containing the several angles respectively into the product of the *classes* of the curves touched by the several sides respectively; or, say, it is equal to twice the product of the orders of the angle-curves into the product of the classes of the side-curves. But when several of the curves become one and the same curve, and in particular when the angles are all of them situate in and the sides all touch one and the same curve, it is a much more difficult problem to find the number of polygons. The solution of this problem when the polygon is a triangle, and for all the different relations of identity between the different curves, is the object of the present memoir, which is accordingly entitled "On the Problem of the In- and Circumscribed Triangle;" the methods and principles, however, are applicable to the case of a polygon of any number of sides, the method chiefly made use of being that furnished by the theory of correspondence.

IV. "On the Unequal Distribution of Weight and Support in Ships, and its Effects in Still Water, in Waves, and in Exceptional Positions on Shore." By E. J. REED, C.B., Vice-President of the Institution of Naval Architects. Communicated by Prof—
G. G. STOKES, Sec. R.S. Received December 31, 1870.

(Abstract.)

The object of this paper is to bring within the grasp of calculation what the author considers a much neglected division of shipbuilding science and art, by investigating the actual longitudinal bending- and shearing-strains to which the structure is exposed in ships of various forms under the varying conditions to which all ships are more or less liable. The weakness exhibited by many ships has long pointed to the necessity of further investigation in this direction; and two modern events (the use of iron and steel in shipbuilding, and the introduction of armoured ships) have added much to the urgency of the inquiry.

After glancing briefly at the state of the question as presented in the writings of Bouguer, Bernoulli, Euler, Don Juan D'Ulloa, Romme, Dupin,

and Dr. Young, the author proceeds to show that the introduction of steam as a propelling agent, and of largely increased lengths and proportions for ships, has brought about a comparative distribution of weight and buoyancy very different from that which those writers contemplated. He has taken the cases of three or four typical modern ships, and has had the relative distributions of the weight and buoyancy very carefully and fully calculated and graphically recorded. Owing to the great labour involved, only the most meagre and unsatisfactory attempts to measure and exhibit the actual strains of ships had previously been made; and the author's results are wholly unlike any that have before been worked out and published. The first case is that of the royal yacht 'Victoria and Albert,' which represents the conditions of long fine-lined paddle-steamers, with great weights of engines, boilers, and coals concentrated in the middle, combined with very light extremities. The second case is that of the 'Minotaur,' which represents long fine-lined ships with great weights distributed along their length. The iron-clad 'Bellerophon' is the third case, representing shorter ships with fuller lines and very concentrated midship weights; and the last case is that of the 'Invincible' class, in which the weights of armour &c. are still more concentrated. All these ships are divided into very numerous short lengths; and the weight of hull, weight of equipment, and buoyancy or displacement of each short length are separately calculated, curves of weight and buoyancy being constructed from these items used as ordinates. A third curve, of which the ordinates are the differences between the curves of total weight and of displacement, known as the curve of loads, is constructed. By summing the ordinates, or calculating the areas of this curve, from point to point, a curve of shearing or racking forces is formed; and by employing the products of the areas of the curve of loads (taken step by step) into the distances of their centres of gravity from one end as ordinates of a new curve, a curve of bending-moments is constructed.

These operations are performed for all the ships previously named, first when they are floating in still water, next when they are respectively floating on the crests of waves of their own lengths, and thirdly when they are floating in the hollow of two adjacent waves of those lengths. The maximum breaking-strains of all the ships when supported on shore, first at the extremities and next at the middle, are also calculated, and compared with the still-water and sea strains.

In considering still-water strains, the author shows that remarkable contrasts of strain occur between ships light and laden, and that the theories of former writers on the subject require to be greatly modified. In some cases the breaking-strain is increased as the ship is lightened. In discussing the shearing-strains, he points out that the sections of maximum shearing-strain in a ship coincide with the balanced or "water-borne sections" (at which the weight and buoyancy are equal), and that in most ships the number of these sections is equal. The position of absolute

maximum shearing-force occupies very different positions in different types of ship. Sections of zero shearing-force coincide with "sections of water-borne division," on either side of which the weight balances the buoyancy; and their number is usually odd. He afterwards shows that maximum and minimum bending-moments are experienced by sections of water-borne division, and that between two sections of maximum "hogging"-moment there must fall either a section of minimum hogging-moment or a section of maximum "sagging"-moment, and that it is an error to suppose (as all former writers on the subject have done) that the absolute maximum bending-moment falls amidships. In the 'Victoria and Albert' the last-named moment is in the forebody; in the 'Bellerophon' and 'Audacious' it is in the afterbody. The effect of the horizontal fluid pressure in the longitudinal bending-moments is also discovered, and shown to be important.

The dynamical aspect of the question—showing the strains brought upon ships at sea—is admitted to be both the more difficult and the more important. In discussing the strains, the author calculates them approximately under the following assumptions:—(1) That for the moment the effect of the ship's vertical motion may be neglected. (2) That for the moment the ship may be regarded as occupying a position of hydrostatical equilibrium. (3) That the methods of calculating bending- and shearing-strains previously used for still water may be employed here also, in order to approximate to the momentary strains. The following particulars of the 'Minotaur' and 'Bellerophon' floating on the crests and in the hollows of waves of their own length respectively and of proportionate heights, illustrate the results to which the calculations before named have led for those ships.

On Wave-crest.

	'Minotaur.'	'Bellerophon.'
Excess of weight forward	1,275 tons.	445 tons.
" " aft	1,365 "	555 "
" buoyancy amidships.	2,640 "	1,000 "
Maximum shearing-strain	1,365 "	555 "
" bending-moment ..	140,300 foot-tons.	43,600 foot-tons.

In Wave-hollow.

Excess of buoyancy forward ..	685 tons.	640 tons.
" " aft	695 "	600 "
" weight amidships ..	1,380 "	1,240 "
Maximum shearing-strain	695 "	640 "
" bending-moment ..	74,800 foot-tons.	48,800 foot-tons.

The strains of ships supported on shore, first at the extremities and then at the middle, are next investigated. The following Table gives the ap-

proximate quantitative values of the shearing-forces and bending-moments obtained for the three ships, ‘Minotaur,’ ‘Bellerophon,’ and ‘Victoria and Albert’ :—

	‘Minotaur.’		‘Bellerophon’		‘Victoria and Albert.’	
	Shearing-force.	Bending-moment.	Shearing-force.	Bending-moment.	Shearing-force.	Bending-moment.
	Displace-ment.	Displace.× length.	Displace-ment.	Displace.× length.	Displace-ment.	Displace.× length.
In still water	$\frac{1}{22}$	$\frac{1}{88}$	$\frac{1}{33}$	$\frac{1}{178}$	$\frac{1}{18}$	$\frac{1}{139}$
On a wave-crest	$\frac{1}{7}$	$\frac{1}{28}$	$\frac{1}{13}$	$\frac{2}{97}$	$\frac{1}{11}$	$\frac{1}{43}$
In a wave-hollow	$\frac{1}{14}$	$\frac{1}{53}$	$\frac{1}{11}$	$\frac{1}{43}$	$\frac{1}{8}$	$\frac{1}{23}$
Supported at the extremities.	$\frac{1}{2}$	$\frac{1}{7}$	$\frac{1}{2}$	$\frac{1}{7}$	$\frac{1}{2}$	$\frac{1}{8}$
Supported at the middle	$\frac{1}{2}$	$\frac{1}{10}$	$\frac{1}{2}$	$\frac{1}{10}$	$\frac{2}{3}$	$\frac{1}{12}$

February 16, 1871.

General Sir EDWARD SABINE, K.C.B., President, in the Chair.

The following communications were read :—

I. “On some of the more important Physiological Changes induced in the Human Economy by change of Climate, as from Temperate to Tropical, and the reverse” (concluded)*. By ALEXANDER RATTRAY, M.D. (Edinb.), Surgeon R.N., H.M.S. ‘Bristol.’ Communicated by Mr. BUSK. Received January 6, 1871.

IV. *The influence of Tropical Climates on the Kidneys and Skin.*

None of the organs of the body are more visibly affected by great changes of climate than these, and their secretions, the urine and perspiration. As with the lungs† and other internal viscera, the congestion of the kidneys lessens, while that of the skin increases, when the blood is attracted to the surface by heat. The reverse happens when it is driven inward by cold. This involves their special and vicarious, waste-product and water-excreting functions alike. In the tropics the skin doubtless excretes much of the water thrown off by the kidneys and lungs in colder regions, as well as the nitrogen and carbon of the former, and carbonic acid of the latter. The elimination of surplus water, one of the most important uses of all of the four great depurating organs, is largely effected by these two. Their intimate relation in this office in cold latitudes is already known. We shall here attempt to show what it is in the tropics.

* Continued from Proceedings of the Royal Society, June 16, 1870, vol. xviii. p. 529.

† Ibid. p. 523.

The following experiments were made on myself (wt. 39), on a voyage from England to Bahia (lat. 11° S.), between June and September 1869. During 24 days, from Plymouth to the thermal Equator, drink (tea or coffee) being limited to 39 oz. daily, the urine gradually decreased from 39 to 30 oz., which merely proved that in semitropical, as in temperate climates, free fluid is chiefly thrown off by the kidneys, and that this diminishes as the heat increases.

The following Table gives the results of the two subsequent days, while passing through the equatorial doldrums or greatest heat, the drink being suddenly increased to 88 oz. daily:—

TABLE I.—To show the Urine excreted at the Equator.

Locality.	Average temp. F.	Date.	9 A.M. Night urine.		9 P.M. Day urine.		Totals.	
			Quantity.	Sp. gr.	Quantity.	Sp. gr.	Quantity.	Sp. gr.
Equatorial doldrums off the African Coast, lat. 11° N.	80	July 14	16	15	20½	11	36½	14½
	81	July 15	16½	13	21	8	37½	10½

Thus nearly 37 oz. were excreted by the kidneys, leaving 51 oz. to be accounted for. Now the bile is scarcely if at all increased in the tropics, so that the liver gives little aid. Dalton* gives $\frac{1}{8}$ of the drink as the average thrown off in this form and by the bowels in temperate latitudes.

Taking this for the tropics also, allowing a little increase for bile, we have 4·4 oz. And reducing the water exhaled by the lungs in the temperate zone (which, according to Dalton, is $\frac{1}{3}$ of the drink or 22 oz.) by the same ratio as the respired air, viz. 11 per cent. or 2·42 oz., we have 19·58 oz. as that for the tropics. The sum of these two is 23·98 oz. Then the

51 oz. not thrown off by the kidneys
—23·98 oz. excreted by the lungs and bowels

gives 27·02 oz. for the skin to exhale.

So that the 88 oz. free fluid were got rid of thus:—

Urine 37 oz., skin 27·02 oz., lungs 19·58 oz., faeces 4·4 oz.

Had the water in the solid ingesta been reckoned, a difficult matter on shipboard, the experiment would have been more satisfactory. But this gives a fair approximation, inasmuch as any excess from this source would only have gone to increase the perspiration.

The relative excretion of free fluid by the skin, kidneys, lungs, and bowels, thus, differs in temperate and tropical latitudes, as they doubtless do in arctic regions (Table II.).

* Hooper, 'Physicians' Vade Mecum.'

TABLE II.—To show the relative excretion of free fluid in Temperate and Tropical latitudes.

Organ.	Temperate zone*.	Tropics.
Kidneys	(about) $\frac{\text{oz. per cent.}}{\frac{1}{2}} = 45.25 = 59.5\frac{1}{2}$	(about) $\frac{\text{oz. per cent.}}{\frac{1}{2}} = 37 = 42.01$
Lungs	(somewhat more than) $\frac{1}{2} = 20.50 = 26.97$	(somewhat more than) $\frac{1}{2} = 19.58 = 22.25$
Skin	(rather less than) $\frac{1}{12} = 6.50 = 8.55$	$\frac{1}{12} = 27.02 = 30.7$
Bowels	(about) $\frac{1}{25} = 3.75 = 4.03$	$\frac{1}{12} = 4.4 = 5.$

While the urine thus decreases from $59\frac{1}{2}$ to 42 per cent., the perspiration rises from $8\frac{1}{2}$ to 30 per cent., there being a slighter fall of $4\frac{1}{2}$ per cent. from the lungs, and a trifling rise from the bowels. The kidneys are thus the chief eliminators of surplus water in the tropics as in temperate regions; but in the former it is the skin, as in the latter it is the lungs that rank next. If suddenly stressed, however, by excessive imbibition, and the safety-valve action of the kidneys or skin be brought into play, these proportions doubtless differ. Will they hold good for permanent residents in the tropics, foreign or native?

The increased perspiration in the tropics or in artificial heat, and diminished urinary and pulmonic water-excretion by 22 per cent., is equal to a proportionate increase in the cutaneous circulation and corresponding withdrawal of blood from the kidneys to the extent of $17\frac{1}{2}$ per cent., and lungs of $4\frac{1}{2}$ per cent. Moreover this diminished exhalation of watery vapour from the lungs, by vicarious action of the skin, still further decreases the amount of blood circulating through them, already shown to be reduced by 12.24 per cent., or 16.62 fl. oz. by a diminished excretion of carbon†. The total decrease in the lung circulation is thus:—

$$\begin{aligned}
 &16.62 \text{ fl. oz.} \\
 &+ 6.42 \text{ ,, (4.72 per cent.)} \\
 &\hline
 &= 23.04 \text{ fl. oz. as the total permanent withdrawal of blood} \\
 &\quad \text{from the lungs by an average temp. of } 80\text{--}83^{\circ} \text{ F.}
 \end{aligned}$$

These facts appear highly interesting in the etiology of these and other important internal and external organs, as well as hygienically and therapeutically suggestive.

The following results of the entire voyage from Bahia to England on a

* Hooper, 'Physicians' Vade Mecum.' In Dalton's experiment the amount of free fluid drunk was 76 oz., and in the above 88 oz. daily. The proportionate results, however, are the same in both.

† Proc. R. S. 1870, vol. xviii. p. 515.

daily allowance of 88 oz. free fluid (Table III.), will show that this *passu* increase and decrease in the perspiration and urine are by no means uniform on going to or quitting the tropics, but oscillate considerably in all latitudes, both in quantity and contained solids, even in adjacent days.

TABLE III.—To show the quantity and contained solids of the Urine in a voyage across the tropics of 34 days.

Date.	Average temp. F.	Locality.	Urine.						Contained solids.
			Morning.		Evening.		Totals.		
			Quantity.	Sp. gr.	Quantity.	Sp. gr.	Quantity.	Sp. gr.	
1860.		Bahia.							grains.
Aug. 8.	76½	Lat. 13 S.	44	5	38	8	82	6.30	503.97
" 9.	77	" 13.5 "	38	5	42	6	80	5.52	400.61
" 10.	77½	" 13.27 "	36	6	46	5	82	5.44	419.85
" 11.	76½	" 11.31 "	19	10	46	5	65	6.46	399.75
" 12.	77	" 8.46 "	44	4	35	7	79	5.32	404.40
" 13.	78	" 5.43 "	42	4	32	8	74	5.72	378.80
" 14.	78	" 3.0 "	36	5	42	4	78	4.40	319.17
" 15.	77½	" 0.38 "	42	7	31	4	73	5.72	373.77
" 16.	78½								
" 17.	79	" 1.37 N.	37	4	20	7	60	5.31	368.65
" 18.	79½	" 3.64 "	42	5	28	7	70	5.8	358.41
" 19.	81	" 5.40 "	20	9	39	6	50	7.01	423.70
" 20.	81½	" 7.17 "	20	9	29	8	49	8.40	494.61
" 21.	80	" 8.35 "	32	4	30	4	71	4	290.53
" 22.	81	" 10.52 "	14½	10	38	4	52	5.71	266.25
" 23.	81	" 12.1 "	27	7	35	5	62	5.87	317.45
" 24.	82½	" 15.10 "	41	4	38	4	70	4.0	323.26
" 25.	78½	" 16.42 "	22½	7	21	9	43½	7.96	361.56
" 26.	78½	" 18.42 "	19	4	37	4	50	4.0	229.1
" 27.	78	" 21.13 "	27	4	23	9	50	6.3	301.52
" 28.	78½	" 23.52 "	24	8	35	3½	50	5.33	302.00
" 29.	77	" 26.15 "	26½	7	32½	4	50	5.34	302.06
" 30.	78	" 28.44 "	25	6	62	3	87	3.66	355.95
" 31.	79	" 28.59 "	19	6	53	4	72	4.62	294.63
Sept. 1.	79	" 29.51 "	24	4	53	4	77	4.0	315.07
" 2.	76	" 30.18 "	12	11	37	4	49	5.75	250.80
" 3.	76½	" 32.6 "	18	8	44	4	62	5.16	317.45
" 4.	74½	" 33.27 "	13	7	53	4	66	4.50	270.06
" 5.	73½	" 34.58 "							
" 6.	74	" 36.20 "	20	8	51	4	71	5.12	362.47
" 7.	73	" 38.12 "	15	6	34	4	49	4.61	200.50
" 8.	72	" 38.58 "	17	5	35	5	50	5.0	256.26
" 11.	66	" 44.22 "	25	5	48	5	73	5.0	371.68
" 12.	65	" 46.5 "	14	8	56	5	70	5.6	367.36
" 13.	65	" 48.18 "	22	8	27	6	63	5.45	316.52

Thus on three consecutive days, taken at random, we find 49, 71, and 52 fl. oz., with 494, 290, and 266 grains of solids. The decrease in the latter, as well as in the fluid, is due partly to the reduced ingesta, and partly to the vicarious action of other organs, especially the skin and liver—and

doubtless involves not only the urea and chloride of sodium*, but all of its ordinary ingredients. Both would be far more regular if the system could be kept day by day in strictly similar conditions as to exercise, clothing, draughts, food, and especially drink—a difficult matter at sea, though possible on shore. So that by limiting the drink and increasing it only as thirst prompted, the quantity of urine would keep at a uniform and perhaps healthier standard. The individually different quantities necessary to accomplish this may be easily ascertained. Thus, allowing 25 oz. free fluid to be what my system requires daily in the average temperature of London (50° F.), the addition of 1 fl. oz. for every degree above, or its deduction for every degree below that, would keep the urine pretty equable, even though its specific gravity and solids might alter (Table IV.).

TABLE IV.—To indicate the daily quantity of drink necessary to keep the Urine nearly alike in Temperate and Tropical latitudes.

Temperature of air (F.)	30°	40°	50°	60°	70°	80°	90°
Free fluid required	5 oz.	15 oz.	25 oz.	35 oz.	45 oz.	55 oz.	65 oz.

This fact was proved by an experiment (of which Table V. is a synopsis) made in the Pacific in 1860–61, during a passage from Valparaiso (lat. 33° S.) to Vancouver (lat. 48° N.), when the drink was not kept uniform throughout as in Table III., but increased or decreased, as here indicated, with the desire.

TABLE V.—To contrast the Urine at the Equator and North and South Temperate Zones.

	Specific gravity of 7 cases.	Quantity in 1 case.
Average of 7 days furthest south (lat. 33°), temp. 68° F....	1018½	oz. 36
" " near equator (lat. 5°), " 78° F....	1018½	45·3
" " furthest north (lat. 53°), " 58° F....	1017½	44·3

Here both the quantity and specific gravity increased somewhat; so that the urine is perhaps not so often or much diminished in the tropics as usually believed. It is so when the drink is stinted, and when, though ample, it is not increased and decreased with the temperature (Table III.); but when this is done it remains pretty uniform (Table V.), as it often does even when taken in excess. It is not so much the nephritic as the cutaneous secretion which alters with variations in the amount of drink in the tropics, and in temperate climates the reverse. The functionally excited skin acts as a

* Dr. Forbes Watson and Becker, as quoted in Parkes's 'Practical Hygiene.'

safety-valve for the kidneys in warm, as the latter do for the former in colder ones. While the perspiration depends much on the temperature, the urine is most influenced by the drink. Although heat, or its absence (cold), is thus the chief agent in causing these fluctuations, the humidity, velocity, &c. of the air are not altogether negative. The first acts by stimulating or checking the sudatory glands, and all three by favouring or opposing evaporation. Frequent change of climate tends to develop the ordinary and safety-valve range of action in both organs. In these facts lie several important hygienic and therapeutic indications for the tropics, with a view to prevent or lessen distressing hyperæmia of the skin and excessive perspiration, both the result of undue imbibition, and the latter highly dangerous when suddenly checked, and a frequent cause of disease. By them the reason of the efficacy of tropical, and especially subtropical climates in the prevention when imminent, and cure or relief when actually present, of many diseases of internal organs, not of the abdomen alone, but of thoracic ones, is explained. The sanatory hæmatic and secretive derivative action of natural (tropical) and artificial heat has been already pointed out with regard to the lungs*. Might not the practical physician more frequently act on this hint as to the means and extent by which both the circulation and the function of diseased or over-taxed internal organs may be relieved by thus transferring their blood-current and secretion to sounder ones? Is not this great and general law of a derivation of blood from internal to external organs under heat, and the reverse under cold, the soundest and most philosophical basis on which to erect a new, safe, satisfactory, and permanent system of therapeutics and hygienics?

V. *The Influence of Tropical Climates on the Weight and Strength.*

Besides the already discussed functional, vascular, and other changes in the lungs, skin, kidneys, and other organs of *vegetable* life, which follow a transition from temperate to tropical climates, various phenomena affecting those of *animal* life are also common—*e. g.* languor of body and brain, and generally a loss of weight. More tardy and less evident, but equally worth study, these are not due, like the former, to the general diversion in the blood-current from internal to external parts, but to changes in the blood itself and the tissues which it nourishes, to be hereafter investigated.

Occasionally an individual fattens on going to the tropics, and, instead of losing, gains health and strength. Again, a corpulent person may decrease considerably in weight, while his health, so far from impairing, actually improves. But such cases are exceptional, and, doubtless, consist merely in vitally unimportant fluctuations in the adipose tissue; and as a rule the issue includes a loss in both respects, which, if not disease, is closely allied to it. An opposite result usually follows a contrary change of climate.

* Proc. R. S. 1870, vol. xviii. p. 520.

The following experiments to illustrate this were made in H.M.S. ‘Salamander,’ during a voyage of five months to, and a subsequent stay of three years on the east coast of Australia, while making triannual trips between Sydney (lat. 34°) and Cape York, Torres Strait (lat. 10½° S.), a distance of 1700 miles in a nearly north and south direction. The crew numbered 209, their ages being :—

between 15 and 25 (period of growth)	129	(61·72 per cent.).
„ 25 „ 35 (adult age)	63	(30·14 „).
„ 35 „ 45 (1st period of decline)	16	(7·66 „).
„ 45 „ 55 (2nd „ „)	1	(0·48 „).

Thus 192 (91·86 per cent.) were under thirty-five, which may be considered the prime of life among seamen; while the whole were healthy. They were weighed as far as possible in the same clothes, and between 6 and 7 P.M., about two hours after a light “supper” of tea and biscuit, in order to reduce error from variations in the state of the bowels, stomach, bladder, &c., to a minimum. Their faulty diet, however, unmodified for temperature, and containing salt meat and other hurtful articles, was an unavoidable disadvantage. Fortunately this enables us to observe the effect of an agency far more under control for modification or removal than climate.

TABLE I.—To show the effect of Tropical Weather alone on the weight.

1st weighing, July 2, 1866, on entering tropics, } 108 days, all spent in the tropics.
2nd „ „ October 18, 1866, on quitting tropics, }

Average temperature at Sydney 60° F., at Cape York 82° F.

Salt meat issued on 36 days { Food consumed per man daily.
(with 61 lime-juice days) lb. oz. drs.
Fresh meat issued on..... 72 „ { Average of first week ... 2 5 12¼
„ last „ ... 2 2 9¾

Age.	Total number weighed.	Number and percentage unchanged.	Number and percentage who gained.	Range of gain.	Average gain.	Number and percentage who lost.	Range of loss.	Average loss.
15 to 25	35	per cent. 3= 8·57	per cent. 13=37·14	lb. 1-8	lb. 3	per cent. 19=54·28	lb. 1-14	lb. 5·16
25 „ 35	39	3= 7·7	8=20·51	2-10	3·37	28=71·8	1-12	4·7
35 „ 45	9	2=22·22	1-2	1·5	7=77·77	1-17	5·71
45 „ 55	2	1=50	1=50	5
Totals and percentages.	85	7= 8·24	23=27·06	1-10	3	55=64·71	1-17	5
		30=35·3 per cent.						

Table I. shows the effect of 3½ months’ exposure to an average temperature of 82° F. towards Torres Strait. Of 85 weighed, 64½ per cent. had lost flesh to an average of 5 lbs. Though greatest among the adults (71 per cent.), and especially the higher ages (77½ per cent.), it was large even among the juniors, of whom 54 per cent. instead of growing, lost con-

TABLE II.—To show the effect of Tropical Climate and Salt-meat Diet on the weight.

Average temperature at Sydney 69° F., at Cape York 84° F.

Salt meat issued on	24 days	{	Food consumed per man daily.			
(with 28 lime-juice days)				lb.	oz.	dra.
Fresh meat issued on.....	4 "		Average of first week ...	2	1	7
			" last "	2	2	9

Age.	Total number weighed.	Number and percentage unchanged.	Number and percentage who gained.	Range of gain.	Average gain.	Number and percentage who lost.	Range of loss.	Average loss.
15 to 25	33	per cent. 3 = 9.09	per cent. 6 = 18.18	lb. 1-10	lb. 4	per cent. 24 = 72.73	lb. 1-10	lb. 4
25 „ 35	33	2 = 6.06	2 = 6.06	1-4	2½	29 = 87.87	1-10	6
35 „ 45	7	1 = 14.3	6	6 = 85.71	2-12	5
45 „ 55	3	3 = 100	3-10	5-6
Totals and percentages.	76	5 = 6.58	9 = 11.84	1-10	3.88	62 = 81.58	1-12	4.1
		14 = 18.42 per cent.						

Table II., the results of another northward cruise, shows how much this loss of weight is increased when the diet is one of salt meat*. The season being the same (S.E. monsoon), though the exposure was shorter by 80 days, no fewer than 81 per cent. lost to an average of 4 lbs.—this, among the boys and youths, being larger than before, even though their food was increased; which proves that a diet like this, not only highly salted but too nitrogenous for warm climates, adds materially to the injurious influence of tropical weather at all ages. After a more prolonged stay at Cape York (one year), eleven marines, fed on a mixed, fresh and salt-meat diet, had lost weight to the average extent of $11\frac{9}{11}$ lbs.

* *i. e.* The ordinary sea dietary, in which 1 lb. of salt meat, beef and pork alternately, forms the chief part of the dinner.

TABLE VI.—To show the influence of Temperate Climates &c. on the weight.

1st weighing, September 14, 1864, near Sydney,

2nd ,, November 25, 1864, after leaving Sydney,

} 72 days, all spent in the temp. zone.

Average temperature at Sydney 65° F.

Salt meat issued on	35 days	Food consumed per man daily.		
(with 20 lime-juice days)		lb. oz. drs.		
Fresh meat issued on.....	37 ,,	Average of first week ...	2	8 3 $\frac{4}{7}$
		,, last ,, ...	1	10 1

Age.	Total number weighed.	Number and percentage unchanged.	Number and percentage who gained.	Range of gain.	Average gain.	Number and percentage who lost.	Range of loss.	Average loss.
		per cent.	per cent.	lb.	lb.	per cent.	lb.	lb.
15 to 25	43	3= 6·98	35=81·39	2-11	5·91	5=11·63	1-6	3
25 ,, 35	32	4=12·5	26=81·25	2-14	6·5	2= 6·25	2-3	2·5
35 ,, 45	8	7=87·5	1-15	7·14	1=12·5	1	1
45 ,, 55
Totals and percentages.	83	7= 8·43	68=81·93	1-15	6·3	8= 9·64	1-6	2·62
		75=90·36 per cent.						

Table VI., in strong contrast to the above, shows how much and rapidly the system rebounds under an opposite change of climate, and when removed from excessive warmth into a healthy temperate climate, with a fresh meat and vegetable diet, light work, frequent leave, &c. Thus after a 54 days' stay at Sydney in spring, notwithstanding the debilitating effect of 35 salt meat days before and after the experiment, no fewer than 90 per cent. had either gained flesh or lost nothing, the average gain being large (6 lbs.). In the 9½ per cent. who lost, this was probably due, as it occurred among the juniors, to those excesses so common after long confinement on board.

Thus, during the three years over which these triannual trips from Sydney to Cape York extended, the weight of the crew was continually oscillating, increasing at the former, and again decreasing on returning to the tropics. Frequent, sudden, and great changes of temperature and climate like this, are doubtless fertile causes in undermining the constitution and inducing premature old age. But for the re-invigorating influence of the periodic return to cool weather, many more would have succumbed to broken health. As it is, Table VII. shows that after 1½ years 44 per cent. of those who originally went out in the ship had lost flesh, while other evidence showed that the health and strength of all had declined, there being moreover no proof of the occurrence in any of that doubtful event, acclimatization. The appetite and consumption of food had also diminished from the same cause.

Very different was the effect of the 44 days' continuation of the voyage to Sydney along the 40th parallel of south latitude, after a health-infusing stay of fifteen days at Simons Bay (Table X.). Although the usual sea dietary was continued, the exit from the tropics was an evident relief to the system, which, but for the diet, would have retained its vigour throughout. As it was, only 26, =19·69 per cent., lost flesh slightly. All the boys, who had the strong vital resilience of youth in their favour, gained in weight, and also the younger men, while the seniors lost. An increase in the ingesta of 7 drams daily towards the end of the period is too trivial to have influenced these results. This shows how long the system will ward off the scorbutic diathesis when opposed by no other serious adverse agency, provided lime-juice is given as a prophylactic. The increased sick-list and intensity of the ailments, however, towards the end of the period, show that the immunity was passing off. The difference in the percentage of those who lost flesh in this and the former part of the voyage (Table IX.), is evidently the effect of climate, and indirectly confirms Tables I. and II.

To the bracing effect of the S. temperate zone we must chiefly ascribe the recovery of the crew. Wasted while crossing the equator, only 29 per cent. were below their original weight in England on arrival at Sydney. This loss, though evident among the juniors, was chiefly among the adults and older men (Table XI.). The superior health and efficiency of a crew in cool climates is an evident indication in planning long voyages. And we have only to recollect the position and direction of the chief winds and ocean-currents usually followed, to see how much these favour the maintenance of health as well as rapidity of progress.

TABLE XI.—To show the effect of a voyage of 144 days on the weight.

Average temperature in England 50° F., Equator 88° F., Sydney 62° F.

1st weighing, January 9, 1864, in England, } 144 days { 27 in the tropics,
2nd ,, June 2, 1864, near Sydney, } { 117 in temp. zone.

Salt meat issued on 110 days	Food consumed per man daily.
(with 50 lime-juice days)	
Fresh meat issued on ... 34 ,,	
	lb. oz. dra.
	Average of first week ... 1 12 13½
	,, last ,, ... 2 5 11½

Ago.	Total number weighed.	Number and percentage unchanged.	Number and percentage who gained.	Range of gain.	Average gain.	Number and percentage who lost.	Range of loss.	Average loss.
		per cent.	per cent.	lb.	lb.	per cent.	lb.	lb.
15 to 25	76	5=6·58	52=68·42	1-18	5·73	19=25	1-25	5·47
25 „ 35	42	3=7·14	25=59·52	1-15	5·76	14=33·33	1-15	5·85
35 „ 45	12	1=8·33	6=50	3-7	5	5=41·66	1-14	7·2
45 „ 55
Totals and per-centages.	130	9=6·92	83=63·85	1-18	5·69	38=29·23	1-25	5·84
		92=70·77 per cent.						

The passage to Cape York, however, again increased the general symptoms of an impaired physique (Table XII.), thus proving the existence of a constant ebb and flow in the state of health during long voyages.

TABLE XII.—To show the effect of a voyage of 230 days on the weight.

Average temperature, England 50° F., Equator 88° F., Sydney 62° F., Cape York 80° F.

1st weighing, January 9, 1864. England, } 230 days { 50 in the tropics,
2nd " August 10, 1864, Cape York, } { 180 in temp. zone.

Salt meat issued on 156 days { Food consumed per man daily.
 (with 73 lime-juice days) lb. oz. drs.
Fresh meat issued on ... 74 " { Average of first week ... 1 12 13½
 " last " ... 2 8 7 |

Age.	Total number weighed.	Number and percentage unchanged.	Number and percentage who gained.	Range of gain.	Average gain.	Number and percentage who lost.	Range of loss.	Average loss.
		per cent.	per cent.	lb.	lb.	per cent.	lb.	lb.
15 to 25	61	7 = 11·48	18 = 29·51	1-19	7·11	36 = 59·01	1-32	6·1
25 " 35	35	1 = 2·86	8 = 22·86	1-16	4·87	26 = 74·28	1-22	6·54
35 " 45	7	2 = 14·29	1-2	1·5	5 = 71·43	6-23	13·8
45 " 55
Totals and percentages.	103	8 = 7·77	28 = 27·18	1-19	5·36	67 = 65·05	1-32	6·83
		36 = 35 per cent.						

Thus, though 55 days in all were spent at Madeira, Simons Bay, Sydney, and Brisbane, on a health-giving fresh meat and vegetable diet (see Table VI), and lime-juice freely given, the monotony and confinement of 175 days at sea, with 156 salt-meat days, a double exposure to the tropics, and frequent changes of temperature, had increased the number who had lost weight since leaving England to 65 per cent.—a decided index of failing health and near approach to disease, and perhaps the scorbutic climax.

Similar fluctuations in weight, subsequently observed in the larger crew of H.M.S. ‘Bristol’ during a voyage of 88 days from England to Bahia (lat. 11° S.) and back, are equally interesting and instructive (Table XIII).

Thus the warm weather and salt-meat diet of the first 57 days caused 85 per cent. to lose weight, which rose to 88 per cent. as the time lengthened to 88 days. The return to the north temperate zone, however, speedily reduced this to 47 per cent. As the diet was the same, the latter event must have been purely climatic—an opinion confirmed by the subsequent effect of 28 days’ harbour-life in England, when the number who lost went down to 10·7 per cent., no fewer than 89 per cent. regaining flesh and more than making up for the previous loss.

TABLE XIII.—To show the effect of climate and diet on the weight of an adult crew, June 18 to September 14, 1869.

Average temperature, England 60° F., Equator 77° F., Bahia 76° F.; England (return) 60° F.

Remarks.	Total number weighed.	Number and percentage who gained or did not lose.	Range of gain.	Average gain.	Number and percentage who lost.	Range of loss.	Average loss.
Effect of first 57 days of which { 45 in the tropics { 38 on salt meat.	425	per cent. 64 = 15·06	lb. 1-15	lb. 2·58	per cent. 361 = 84·94	lb. 1-28	lb. 5·75
Effect of first 88 days of which { 66 in the tropics { 61 on salt meat.	296	36 = 12·18	4-13	3·5	260 = 87·84	1-30	5·47
Effect of last 31 days of which { 21 in the tropics { 10 on salt meat.	310	164 = 52·93	4-9	2·64	146 = 47·1	4-10	2·81
Subsequent effect of 28 days in harbour (England) on fresh meat and vegetables.	391	349 = 89·26	1-19	4·70	42 = 10·74	1-9	2·66

Table XIV. shows that, while the same prevailed among the ship's boys (age 17 to 20) and naval cadets (age 14 to 17), youth and lighter work &c., have an evident effect in lessening the percentage of loss under such adverse agencies, and increasing the gain under opposite conditions.

TABLE XIV.—To contrast the variations in weight, during long voyages, of the men, boys, and naval cadets, June 18 to September 14, 1869.

Average temperature, England 60° F., Equator 77° F., England (return) 60° F.

England to Bahia (lat. 11° S.), 88 days.	Total number weighed.	Number and percentage who gained or did not lose.	Number and percentage who lost.
Effect of first 57 days of which { 45 in the tropics, { 38 on salt meat.	Men 425 Boys 64 Cadets 60	per cent. 64 = 15·06 28 = 43·74 23 = 38·33	per cent. 361 = 84·94 36 = 56·25 37 = 58·33
Effect of first 88 days of which { 66 in the tropics, { 61 on salt meat.	Men 296 Boys 40 Cadets 58	36 = 12·18 16 = 35 20 = 34·47	260 = 87·84 24 = 60 38 = 65·51
Effect of last 31 days of which { 21 in the tropics, { 10 on salt meat.	Men 310 Boys 41 Cadets 59	164 = 52·93 30 = 73·17 34 = 57·62	146 = 47·1 11 = 26·82 25 = 42·37
Subsequent effect of 28 days in harbour (England) on fresh meat and vegetables.	Men 391 Boys 34 Cadets 28	349 = 89·26 30 = 90·58 28 = 100	42 = 10·74 4 = 11·76

Thus 85 per cent. of the men lost flesh during the first 57 days, but only 56 per cent. of the boys, and 58 per cent. of the cadets. Again, during the first 88 days the percentages were—men 88, boys 60, cadets 65. Further, while 53 per cent. of the men began to recover weight on reentering cool weather, 73 per cent. of the boys and 58 per cent. of the cadets did the same. Lastly, in England, while 89 per cent. of the men gained, among the boys we find 90½ per cent., and among the cadets 100 per cent. The advanced age, greater strength, and rougher early life of the boys enabled them to bear the voyage better, and recover sooner under genial agencies than the younger delicately-reared cadets. On the other hand, a generous diet and better regulated life caused the latter to increase more in England. Under favourable conditions, as to climate, diet, &c., the weight of men, and particularly boys, should not fluctuate thus. Nor can such changes be salutary. As a rule adults, with fully developed frames, should remain pretty stationary in weight. Boys, however, should increase not only in weight, but in height and breadth of chest. For the former to emaciate, or the latter to grow taller and broader, while the weight remains the same or lessens, is a sure sign of present or impending mischief. The average of 1½ lb. per week by which these cadets increased at home, may be considered the healthy rate of growth for boys of their age. And we may give Table XV. to show the effect of subsequent longer leave in England on the physique of a larger number of cadets.

TABLE XV.—To show the effect of a healthy diet and climate on the physique of naval cadets, age from 14 to 17 (September and October 1870 ; time 44 days ; temperature 64° F.).

	Total number.	Number and percentage unchanged.	Number and percentage of gain.	Range of gain.	Average gain.	Number and percentage of loss.	Range of loss.	Average loss.
Weight.	52	per cent. 1 = 1·925	per cent. 48 = 92·31	lb. 1–20	lb. 5·93	per cent. 3 = 5·77	lb. 1–2	lb. 1·66
Height ..	54	20 = 37·04	34 = 63	in. ¼–2¼	in. 0·67
Chest ...	not measured.

Thus, of 52 cadets, 93 per cent. either did not lose or gained flesh to the average of 1 lb. per week, while 63 per cent. increased in height, and no doubt in capacity of chest ; but the time was too short to obtain satisfactory results as to this. Obviously, therefore, if cadets are long subjected to influences which retard their growth, even if disease does not ensue, their future strength, both of body and brain, is apt to be impaired ; while ship's boys and young seamen are not likely to become physical athletes, nor adults to retain their vigour as fighting men. These conclusions necessarily apply to all similarly situated.

If we can isolate the effects of tropical weather so as to contrast them with those of other health-impairing agencies, it will be both interesting and practically useful. Table XVI. shows when we find the greatest gain or greatest loss of weight. Life under the healthiest conditions, in which the highest gain (90½ per cent.) and lowest loss (9½ per cent.) occurs, is first given as a standard for comparison and index of what should be aimed at in all latitudes and circumstances.

TABLE XVI.—To compare the effect of climate and other agencies on the weight.

Reference.	Pernicious influences.	Gain or unchanged		Loss	
		Per cent.	Ave- rage.	Per cent.	Ave- rage.
Table VI.	None	90·86	lb. 6·3	9·64	lb. 2·63
„ X.	One (salt meat)	81·05	5	19·69	2·58
„ I.	One (tropical climate)	35·30	3	64·71	5
„ IX.	Two (tropical climate, dry season, and salt meat).	34·78	3·9	65·22	6·39
„ V.	Two (tropical climate, wet season, and salt meat).	23·66	2·8	76·34	7·15
„ III.	Three (tropical climate, salt meat, and hard work).	8·73	3·66	91·26	6·96

We here notice a progressive decrease in the number who gain or do not lose in weight, and necessarily a corresponding increase in the percentage of those who lose, according to the variety and intensity of the adverse agencies. Thus fewest emaciate when the influences are altogether genial, viz. 9·64 per cent. An injurious diet raises this to 19·69 per cent. Under tropical climate it rises to 64·71. Under the latter and salt meat combined, it again rises to 65·22 per cent., and in the rainy season to 76·34 per cent. When, besides this, hard work is undergone, it mounts to 91·26 per cent. The average gain and loss columns show a similar though less regular increase and decrease. Tropical climate is thus by far the most injurious influence; and its effects are materially aggravated by other adverse agencies. [And the Tables show that these facts apply to the junior as well as the senior ages, though occasionally more apparent in the latter.—Feb. 27.]

We must know the nature of these universal and marked changes in the weight, and the tissues involved, before we can decide whether they are physiological or pathological, and, if the latter, satisfactorily direct our hygienic or therapeutic efforts to prevent or remedy them. We cannot ascertain by anatomical or histological investigation; but we may fairly suppose that every or nearly every tissue is more or less implicated—those

which carry on the functions of animal life being most affected, especially such as form the great bulk of the body. It would be difficult to say whether the watery part of the blood and body generally is reduced by excessive perspiration. The osseous system and thoracic and abdominal viscera are probably little changed. The fibrous and gelatinous are perhaps more altered; but it would be difficult to separate this from the change in the fatty muscular and nervous tissues, the three doubtless most of all affected. In warm latitudes less fat is required than in cold ones to keep out cold and generate internal heat or muscular force. Hence nature uses it up in its vital processes, and thus first gets rid of what does not itself play a vital part in the human economy, or materially influence health by its removal, and would only prove an encumbrance. The prevalent languor of body and mind no doubt arise partly from diminished energy in the nervous and muscular tissues; but are they not also, and perhaps principally, due to a decrease in their bulk, similar to that in other tissues? Strength is the manifestation of muscle acted on by nervous influence; and, from several experiments made on the officers and crew of H.M.S. 'Bristol,' strength decreases and increases with the foregoing changes in weight—a fact which goes far to prove that though loss of strength may be partly of nervous origin, the muscular tissue is also largely involved in its production, and is probably both physiologically weakened and physically altered in texture*.

The cause of this reduction in weight in the tropics is threefold:—first, a diminished necessity for surplus fat, which becomes absorbed; second, that peculiar and not easily explained physiological effect of heat, which causes the tissues to decay faster than in cold latitudes; third, diminished lung-work and blood-oxygenation, and thereby an imperfect renewal of tissue. On the other hand, the languor and weakness are due, first, to loss and relaxation of the muscular substance; second, to a similar loss of nervous tone and matter; third, to suboxidation of the blood†, which impairs the activity not only of the muscles, but of the nerve-centres which originate, and nerve-cords which transmit motor and sensory impressions; [and, fourthly, in their early stage, to a reduced supply of their vital stimulant the blood, diverted from the internally situated nerve centres, nerves, and muscles, to the cutaneous surface.—Feb. 27.] The early and primary results of tropical warmth on the tissues are probably chiefly physical and quantitative; but when prolonged, especially if conjoined with an erroneous diet, their composition is affected, and they are also chemical and qualitative.

What are the true bearings and diagnostic value of this closely-allied loss of weight and strength? Are they solely physiological? or when do they become pathological? Do they always, or at what stage do they indicate

* These data were scarcely ample enough for tabulation. The ship's motion, imperfect testing apparatus, and difficulty of finding one equally suited for all men, in whom the best-developed sets of muscles often differ, make this a troublesome inquiry.

† Proceedings of the Royal Society for 1870, No. 122, p. 520.

a loss of vitality or health? If decreased weight originates merely in an absorption of fatty tissue, and no strength is lost, the result is at least not unhealthy. But when other tissues are involved (and it would be difficult to decide when they are, as this doubtless varies even in the same individual), it is then, if not disease, closely allied to it—and certainly an indication of an impaired and debilitated physique, prone to succumb to other morbid agencies, and ultimately to induce premature decay and old age. Physiological in their earlier stage, they soon become of doubtful nature, and finally decidedly pathological. And that there is a special and not merely a general relation between these phenomena and the health appears, first, from the results being so marked, uniform, and generally prevalent; second, from concurrent indications of debility, shown by a progressive increase in the amount and severity of sickness; and, third, by a marked decrease in the loss of weight and strength in the tropics, when some of the agencies which indirectly augment its influence are removed, as will be proved by the following Table, which shows the effect of an improved diet.

During a similar voyage from England to the South Atlantic, in two of Her Majesty's ships, both crews were subjected to a corresponding amount of tropical weather; but the number of salt-meat days in H.M.S. 'Bristol' was twelve fewer than in H.M.S. 'Salamander'*, the result being that in the former the number of those who lost flesh and strength was reduced by 22 per cent.

TABLE XVII.—To contrast the results of two similar voyages on the weight.

	Total number weighed.	Number and percentage unchanged.	Number and percentage who gained.	Range of gain.	Ave- rage gain.	Number and percentage who lost.	Range of loss.	A n k
H.M.S. 'Bristol.' 65 days { 34 in the tropics { 38 on salt meat	379	per cent. 42=11·08	per cent. 172=45·38	lb. 1-12	lb. 2·73	per cent. 165=43·53	lb. 1-13	2
H.M.S. 'Salamander.' 55 days { 34 in the tropics { 50 on salt meat	116	7= 6·03	33=28·45	1-13	4·18	76=65·52	1-23	7

[This was equally apparent among the cadets (Table XVIII.). Thus, of 58, the number who lost weight became reduced from 65½ to 40 per cent. by a removal from the tropics, combined with a limited use of salt meat. The improvement in their growth, as shown by their height and measurement of chest, was equally obvious.

* From a lately introduced issue of preserved meat every third day in the naval dietary.

TABLE XVIII.—To contrast the results of two voyages on the weight of Cadets.

	Number weighed.	Number and percentage who gained or did not lose.	Number and percentage who lost.
A voyage to Bahia of 88 days:— in tropics, 66 days... } on salt meat, 51 „ ... }	58	per cent. 20 = 34·47	per cent. 38 = 65·51
A voyage to the Mediterranean of 100 days:— in tropics, 0 days ... } on salt meat, 5 „ ... }	57	34 = 59·64	23 = 40·35

The general loss of flesh (in other words, absorption of internal tissue) which results from the salt-meat dietary of long voyages, and which is here seen to be so greatly intensified in and by tropical climate, is really the essence and primary stage of scurvy, and corresponds in principle and nature with the visible, external, and superficial breaking down and loss of substance in the phlegmous abscesses, ulcers, &c., still too prevalent in the service, and in its more serious and advanced forms of the dysentery, and putrid ulcer, once so common and fatal; while the intensity, obstinacy, and sometimes the origin of many other local and general diseases frequent among seamen, *e. g.* rheumatism, syphilis, struma, various fevers, continued, contagious and periodic, &c., have doubtless an equally close alliance.—Feb. 27.]

These experiments were carried out in super-oceanic climates. It would be interesting to know how the weight and strength are affected in continental ones, where the range of temperature and humidity &c. are greater, as, for example, when troops are moved from the cool hilly regions of India to its sultry lowlands.

These facts suggest important hygienic and therapeutic indications; for example:—

First. That the tropics, especially during the rainy season, should be avoided by natives of colder latitudes.

Second. That the young, the debilitated, and the diseased should especially shun warm regions.

Third. That none but full-grown healthy adults should go there.

Fourth. That with all, even the latter, a speedy exit should be made therefrom, when great loss of flesh and strength give warning of approaching disease.

Fifth. That such injurious agencies as may increase the weakening or disease-inducing influences of tropical climates, of themselves irremediable, should be avoided, *e. g.* faulty diet, over fatigue, impure air, &c.

Sixth. That, to preserve health, a tropical climate should be frequently changed for the more temperate ones of higher altitudes or latitudes.

VI. *Conclusion.*

The ultimate object of these varied functional and organic changes induced in the human frame by change of climate, is to accommodate it to altered meteorological and other conditions, and assimilate it to those of native races. It is the ease or difficulty with which different varieties of mankind, ages, sexes, and idiosyncrasies become accustomed to this that indicates their capability for what we term acclimatization. [Would not a more intimate acquaintance than we yet possess with the differences in the minute anatomy and functions of the various tissues and organs of these different races and families, and also their correlation and capability or not of assimilation under change of climate, go far to decide the long-vexed questions as to the unity or plurality of species and of creative centres? —Feb. 27.] In these important changes, moreover, especially that in the current of the blood from the interior to the surface of the body on proceeding to the tropics, there is an evident analogy with certain great operations which take place under similar circumstances in the inorganic world. The air and ocean likewise heat as they proceed towards the equator, and finally overflow to form those beneficent winds and sea-currents which play so important a part in the economy of the globe, and influence its hygiene, therapeutics, and etiology, not less than its commerce. And although in these it acts on what may be termed the centre of their circulation, whereas in the human frame it operates on its periphery, the agent in all three is the same, viz. the sun's heat, as is the primary effect, viz. a change in the direction of original currents, as well as the final results, viz. purification and modification of temperature. The general physical and general hygienic and curative schemes of nature are thus evidently connected. Without these phenomena the heat of tropical lands and seas, and cold of other regions, would be intolerable, and that of the skin and body too high or too low for the maintenance of their vitality; while both the air, ocean, and blood would rapidly become impure and unfit to sustain life.

Deriving its first and chief impulse from the heart, the blood merely undergoes redistribution—the current in cold and temperate climates being directed towards internal, and in the tropics towards external organs, especially the skin. In either case it flows from cooler towards more highly heated regions. Is not this vital process, therefore, in this respect also, at least partly akin to the allied phenomena in the air and ocean, and physical as well as physiological? The blood generally being probably somewhat warmer in the tropics than elsewhere, does not the heating of the surface and contents of the turgid cutaneous capillaries act as a *vis à fronte* in inducing it to flow towards and accumulate here, as the warm interior does in cold regions?

II. "On a Registering Spectroscope." By WILLIAM HUGGINS,
LL.D., D.C.L., F.R.S. Received January 14, 1870.

The short duration of the totality of the solar eclipse of December last, led me to seek some method by which the positions of lines observed in the spectrum of the corona might be instantly registered without removing the eye from the instrument, so as to avoid the loss of time and fatigue to the eye of reading a micrometer-head, or the distraction of the attention and other inconveniences of an illuminated scale.

After consultation with the optician Mr. Grubb, it seemed that this object could be satisfactorily accomplished by fixing in the eyepiece of the spectroscope a pointer which could be moved along the spectrum by a quick-motion screw, together with some arrangement by which the position of this pointer, when brought into coincidence with a line, could be instantly registered.

I was furnished by Mr. Grubb with an instrument fulfilling these conditions, and also with a similar instrument with some modifications by Mr. Ladd, in time for the observation of the eclipse.

Unfortunately at my station at Oran, heavy clouds at the time of totality prevented their use on the corona; but they were found so convenient for the rapid registration of spectra, that it appears probable that similar instruments may be of service for other spectrum-observations.

In these instruments the small telescope of the spectroscope is fixed, and at its focus is a pointer which can be brought rapidly upon any part of the spectrum by a screw-head outside the telescope. The spectrum and pointer are viewed by a positive eyepiece which slides in front of the telescope, so that the part of the spectrum under observation can always be brought to the middle of the field of view. The arm carrying the pointer is connected by a lever with a second arm, to the end of which are attached two needles, so that these move over about two inches when the pointer is made to traverse the spectrum from the red to the violet. Under the extremity of the arm fitted with the needles is a frame containing a card, firmly held in it by two pins which pierce the card. This frame containing the card can be moved forward so as to bring in succession five different portions of the card under the points of the needles; on each of these portions of the card a spectrum can be registered.

The mode of using the instrument is obvious. By means of the screw-head at the side of the telescope, the pointer can be brought into coincidence with a line; a finger of the other hand is then pressed upon one of the needles at the end of the arm which traverses the card, and the position of the line is instantly recorded by a minute prick on the card. A bright line is distinguished from a dark line by pressing the finger on both needles, by which a second prick is made, immediately below the other. In all cases the position of the line is registered by the same needle, the second needle being used to denote that the line recorded is a bright one.

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It was found that from ten to twelve Fraunhofer lines could be registered in about 15 seconds, and that, when the same lines were recorded five times in succession on the same card, no sensible difference of position could be detected between the pricks registering the same line in the several spectra.

It is obvious that, by registering the spectra of different substances on the card, a ready method is obtained of comparing the relative positions of the lines of their spectra.

Each spectroscope was furnished with a compound prism, which was made by Mr. Grubb, and gave a dispersion equal to about two prisms of dense glass with a refracting angle of 60° .

Postscript.—I have just learned that in a spectroscope contrived by Professor Winlock for observing the eclipse of December 22, 1870, the positions of the observing-telescope are registered by marks made upon a plate of silvered copper.—February 3, 1870.

February 23, 1871.

WILLIAM SPOTTISWOODE, M.A., Treasurer and Vice-President,
in the Chair.

The following communications were read:—

- I. "On the Mutual Relations of the Apex Cardiograph and the Radial Sphygmograph Trace." By A. H. GARROD, of St. John's College, Cambridge. Communicated by Dr. GARROD. Received January 18, 1871.

A desire to acquire an accurate knowledge of the relation borne by the commencing contraction of the heart to the origin of the primary rise in the pulse at the wrist, led the author to construct an instrument which has enabled him to determine, with considerable accuracy, the mutual relation of these two points, and to demonstrate one or two unexpected results, not altogether without interest.

The cardio-sphygmograph above mentioned consists of a piece of board, 10 inches long by $5\frac{1}{2}$ inches broad, and about half an inch thick, along one side of which a sphygmograph can be laid, as shown in fig. 1. On the opposite side a spring (*a*) like that employed in the sphygmograph is attached to a moveable support (*b*), so that its tension can be modified. To the free end of the spring a small pad (*c*) is fixed, which is in communication with the cardiograph apparatus by means of a silk thread (*d*). This latter instrument consists of a light lever (*e*), a little over 2 inches long, connected to the board first mentioned by a frame (*f*) which is just free from the sphygmograph when the latter is in position. The lever, which is one of the third system, is connected on either side, close to its

fixed end, to two silk threads, one of which (*d*) is attached to the pad and spring above mentioned, and the other to a small spring (*g*) which moves it when it is less acted on by the stronger spring. The apparatus is so arranged that the lever works perfectly when it is so placed as to be above the registering part of the sphygmograph, when the latter is in position. The tip of the lever carries a steel pen (*k*).

Fig. 1.



To use the instrument, the sphygmograph is first fixed on the left arm as usual, the recording paper being adjusted to its place. The arm is then moved until the attached instrument rests on the board first mentioned; and it is maintained in position by certain pegs and holes in the board, which respectively come into contact with the main parts, and receive the projections of the instrument.

The arm and attached apparatus are then moved until the pad of the cardiograph spring is brought into contact with the spot, between the fifth and sixth ribs, at which the heart's pulsation is most marked—the position of the pad in relation to the board having been previously so fixed as to enable this to be done with facility, the whole being maintained in the horizontal position. The contact of the pad with the chest-wall causes the lever to recede; and it is allowed to do so until its pen arrives above the recording-paper, the whole apparatus being steadied by the right hand. When the levers of the two instruments are both found to be moving freely, the watchwork of the sphygmograph is set in action by means of a string, connected at the other end with the stop-block of the train of wheels; and when the recording-paper has run its length, a combined trace is found, as in figure 2.

Fig. 2.



The commencement of the two traces is easily defined with precision;

and as they are both recorded on the same paper, synchronous movements must be at equal distances from the starting-points, and therefore they can be projected on one another. The results obtained by these projections form the subject of this communication.

All the observations were made on the same subject, *ætat.* 24, in good health. They were all made in the sitting posture, as the apparatus could then be held more firmly, or rested on the arm of a chair.

To facilitate description, the following terms and symbols will be employed with regard to pulse-traces.

1. The rapidity of the pulse is symbolically represented by x .

2. The first cardiac interval is that which occurs between the commencement of the systolic rise and the point of closure of the aortic valve, in cardiograph traces. The number of times that this interval is contained in its component beat is represented by y ; and the law as to its length, published elsewhere*, will be assumed; it may be stated thus:

$$xy = 20 \sqrt{x}.$$

3. The first arterial interval is that which occurs between the commencement of the primary rise and the termination of the major fall in arterial sphygmograph traces. The number of times that this interval is contained in its component beat is represented by y' ; and the law as to its length at the radial artery, which is alone considered in this communication, published in the Proceedings of the Royal Society (No. 120, 1870), will be assumed; it may be thus stated:

$$xy' = 47 \sqrt[3]{x}.$$

4. The first cardio-arterial interval is that which occurs between the commencement of the systolic rise in the cardiograph trace and the origin of the main rise in the sphygmograph trace. The number of times that this interval is contained in its component beat is represented by z .

5. The conjugate cardio-arterial interval is that portion of the first cardiac interval which is synchronous with a portion of the first arterial interval. It is therefore the interval between the commencing sphygmograph rise and the point of closure of the aortic valve as represented in the cardiograph trace.

6. The second cardio-arterial interval is that which occurs between the point of closure of the aortic valve and its indication at the artery under consideration.

In commencing to work with the cardio-sphygmograph, measurements were made to find the duration of the first cardio-arterial interval, as it required but a few experiments to prove that the heart commences to contract before the pulse is indicated at the wrist.

By means of compasses, or by superposing one trace on the other, the commencing cardiograph rise was projected on the sphygmograph trace; and the interval between this event and the origin of the radial rise was

* Journal of Anatomy and Physiology. Cambridge, vol. v. Nov. 1870.

then measured into its component beat in each pulsation of the trace, from which the average of the observation was obtained. The results are given in Table I. Column II. ; and in Column III. some of these are expressed in parts of a minute, whereby a better idea can be obtained as to their significance.

TABLE I.

I. $x.$	II. $z.$	III. $\frac{1}{xz}$	IV. $\frac{1}{39\sqrt{x}}$
58	5.2	·003316	·0033649
64	5.083		
70	4.74		
71	4.52		
74	4.50625	·00299	·00298
79	4.3127		
80	4.4437		
81.5	4.1125		
85	4.355		
86	4.2	·002768	·002802
97	4.17		
102	3.885	·002524	·002538
132	3.41	·002222	·002229
154	3.		
170	2.95	·00197	·001957

From these results it is seen that the first cardio-arterial interval is longer in slow than in quick pulses, and that it does not increase as quickly as the pulse diminishes in rapidity ; but that the statement that it varies inversely as the square root of the rapidity is correct, or very nearly so, is rendered evident by comparing Columns III. and IV., in the latter of which the duration of the first cardio-arterial interval is calculated from the formula $xz=39\sqrt{x}$. The chief source of error in these observations is the slight uncertainty in the rate of movement of the watchwork of the instrument, on which the calculation of the rapidity of the pulse depended.

On comparing this equation, namely $xz=39\sqrt{x}$, with the one above referred to as to the relations of the first cardiac interval, namely $xy=20\sqrt{x}$, it is evident that the length of the first cardio-arterial interval is .5128, or just over half that of the first cardiac interval, whatever the rate of the pulse.

This being the case, a more precise method is acquired of verifying the results arrived at ; for by finding the number of times that the first cardio-arterial interval is contained in the first cardiac interval, a constant quantity ought to be the result, which is independent of the rapidity of

the pulse. Table II. contains these measurements ; and it may be seen that, though there is a small range of variation, the numbers are all very near to the theoretical requirement, which is 1.95 ; and their average is 1.983.

TABLE II.

Rapidity of pulse.	Number of times that the first cardio-arterial interval is contained in the first cardiac interval.	Rapidity of pulse.	Number of times that the first cardio-arterial interval is contained in the first cardiac interval.
58	1.95	81.5	1.95
64	2.	83	2.1
69	1.9	84	2.1
70	1.9125	85	1.995
71	1.975	85.5	1.95
72	2.058	86	2.
74	1.975	88.5	2.05
76	1.98	91	2.15
78	1.9	92	1.85
79	1.925	94	1.95
79.5	1.9	97	2.15
80	1.95	154	1.975

It is generally known that in the sphygmograph traces of most slow pulses there is a notch in the first arterial interval, immediately preceding the major fall ; and one of the most marked results of the use of the cardio-sphygmograph is the determination of the fact that the point of closure of the aortic valve at the heart is always exactly synchronous with the lowest part of this notch, or the point of abrupt change of direction in the major fall of the sphygmograph trace. This leads to the almost necessary conclusion that the subsequent slight rise or change in direction of the trace is the result of the simultaneous movement of the whole column of blood produced by the suddenness of the shock of closure of the aortic valve, the secondary rise in the same trace being the more slowly transmitted pressure wave resulting from the same cause.

The slower the pulse the more distinct is this notch ; and by comparing different rapidities, a gradual diminution in its conspicuousness is apparent, it rising higher and higher above the point of termination of the major fall as the pulse is quicker and quicker. When the heart's rate is about 75 in a minute, the notch is halfway down the major descent, and is partially blended with it ; when over 100 a minute, as the aortic valve closes when the ascent is at its maximum, the notch is so blended with the pressure wave as not to indicate itself separately.

In slow pulses, the systolic main rise being quite over when the aortic

valve closes, the shock wave indicates itself by an abrupt but not considerable rise, breaking the very gradual major descent.

This explanation being correct, another means is obtained of checking the results arrived at by the combined instrument; and Table III. Column II. contains a few measurements of the number of times that the conjugate arterial interval is contained in the first arterial interval, as found by measuring the ratio of the interval between the commencing arterial rise and the bottom of the notch in the major fall to the whole first arterial interval. Column III. gives the theoretical results necessitated by the equations given above.

TABLE III.

Rapidity of pulse.	Number of times the conjugate cardio-arterial interval is contained in the first arterial interval,	
	as found from measurement of radial	as calculated (approximately).
37	1.595	1.6
45	1.635	1.625
58	1.69	
59	1.7083	1.72
60	1.74	1.734
68	1.797	1.78

It may be mentioned that the reason why so few of these instances are given, is that there is considerable difficulty in measuring these small intervals into one another with precision; but by practice a very fair estimate can be made of their value, and in all cases they seem to agree with theoretical requirement. The close accordance of the results obtained by this method in very slow pulses, and the calculated results arrived at from facts relating only to quicker ones, tends strongly to establish the correctness of the law given with regard to them.

In Table IV. the lengths, in parts of a minute, of the different intervals referred to in this communication, are given as calculated from the equations on which they have been shown to depend. With regard to the second cardio-arterial interval, a reference to Column VII. will show that it varies very slightly within the range of the heart's action, not being $\frac{1}{2}$ longer in a pulse of 36 than in a pulse of 169 in a minute.

TABLE IV.

I. Rapid- ity of pulse.	II. Length of pulse-beat, in parts of a minute.	III. Length of first cardiac interval, in parts of a minute.	IV. Length of first arterial interval, in parts of a minute.	V. Length of first cardio- arterial in- terval, in parts of a minute.	VI. Length of conjugate cardio-arte- rial interval, in parts of a minute.	VII. Length of second car- dio-arterial interval, in parts of a minute.
36	·027	·0083033	·006428	·0042735	·0040298	·00239821
49	·020408	·00714286	·005813	·003663	·00347986	·00233342
64	·015625	·00625	·005319	·003205	·0030449	·00227425
81	·0123457	·005	·00491356	·0028474	·0027081	·00220546
100	·01	·005	·0046234	·0025641	·0024359	·00218745
121	·0082645	·0045	·004299	·002331	·00221445	·0020847
144	·00694	·00416	·0040486	·0021365	·0020301	·0020185
169	·005917	·003846	·0038485	·0019704	·0018756	·0019729

In conclusion, the following are the results that have been arrived at by the use of the above cardio-sphygmograph:—

1. The first cardio-arterial interval varies inversely as the square root of the pulse-rate.

2. The conjugate cardio-arterial interval varies inversely as the square root of the pulse-rate.

3. The second cardio-arterial interval varies very little with different pulse-rates, but is slightly longer in slower pulses.

4. The depth of the notch in the first arterial interval of the sphygmograph trace occurs at the moment of closure of the aortic valve.

5. There is no definite indication in the sphygmograph trace of the moment at which the arterial systole commences.

II. "On the Thermo-electric Action of Metals and Liquids."

By GEORGE GORE, F.R.S. Received January 13, 1871.

It is well known that the degree of rapidity with which a metal immersed in an acid, alkaline, or saline liquid is corroded varies considerably with the temperature, and that the speed of corrosion usually increases with the heat; also a few experiments have been published (Gmelin's 'Handbook of Chemistry,' vol. i. p. 375) showing that changes of electrical state occur in metals under such circumstances; but a further examination of the relations of the temperature and chemical change to the electrical state has not, that I am aware, yet been made.

In an investigation on the development of electric currents by unequally heated metals in liquids (Phil. Mag. 1857, vol. xiii. p. 1), I found that hot

platinum was electro-negative to cold platinum in liquids of acid reaction, and positive to it in alkaline ones, provided in all cases chemical action was completely or sufficiently excluded. In the present experiments I have endeavoured to ascertain what electrical changes are produced in cases where chemical action more freely occurs, and I have therefore employed not platinum plates, but plates composed of a metal (copper) which is more easily corroded.

To effect the object I had in view, I used the apparatus shown in section in fig. 1, and in perspective, with its wooden support, in fig. 2.

Fig. 1.



Fig. 2.



A and B, fig. 1, are two open thin glass dishes, $6\frac{1}{2}$ inches diameter, and $1\frac{1}{2}$ inch deep, with open necks. The dishes are joined together, water-tight, by a bent glass tube, C, about 1 inch in diameter; and the whole arrangement is securely fixed upon a wooden frame or stand, so that it may be at once placed in an exactly horizontal position, or inverted to pour out its contents. D and E are two dishes of sheet copper of moderate thickness, made from contiguous portions of a sheet of metal to ensure electrical homogeneity in the experiments. Wires of similar metals are attached to the dishes for the purpose of connexion with a galvanometer. A galvanometer, containing about 180 turns of moderately fine copper wire, is sufficiently sensitive for the experiments. The outside of the metal dishes must be made perfectly clean and bright immediately before each experiment.

In using the apparatus it is first set exactly horizontal, and a known and measured volume of the clear liquid to be examined, at the temperature of the atmosphere and sufficient to fill it to the line F F, is poured in; the metal dishes are then steadily placed in the glass vessels and connected with the galvanometer, taking care that no air-bubbles remain beneath them.

As soon as the galvanometer-needles have settled at zero, one of the dishes is quickly filled with boiling water, and the directions and amounts of the temporary and permanent deflections noted.

The following are Tables of results obtained with various liquids, the solutions being diluted in each case to a specified measure by addition of distilled water. Those of the experiments in which 20 ounces of liquid was used, were nearly all of them made with an apparatus in which the connecting-tube C was of somewhat less diameter; and the deflections obtained by that apparatus were less in extent than those obtained with the "new apparatus," because in the latter the conduction-resistance was somewhat less. The values of the deflections given in the Tables are in all cases those of the *temporary* ones; and the liquid used for diluting the solutions was in all cases *water*.

Pure Nitric Acid.

No.	Ounces of strong acid diluted to 20 ozs. with water.		Value of Deflection.	
1. $\frac{1}{16}$	·0045	The hot plate was negative and much acted upon, especially with the stronger mixtures. With the stronger mixtures a little gas was evolved at 60° Fahr., and a large amount directly the heat was applied.
2. $\frac{1}{8}$	·0012	
3. $\frac{1}{4}$	·0039	
4. $\frac{1}{2}$	·0497	
5. 1	·1177	
6. 2	·0356	
7. 3	·0578	
8. 4	·4954	

Pure Hydrochloric Acid.

No.	Ounces of strong acid diluted to 20 ozs.		Value of Deflection.	
1. $\frac{1}{16}$	·0064	The hot plate was positive. The amount of stain upon the hot plate was very small, and was in the form of a dark line at the edge of the liquid.
2. $\frac{1}{8}$	·0330	
3. $\frac{1}{4}$	·1112	
4. $\frac{1}{2}$	·2854	
5. 1	·5731	
6. 2	2·0446	

Chloric Acid.

No.	Ounces of strong acid diluted to 20 ozs.		Value of Deflection.	
1. $\frac{1}{16}$	·0002	The hot plate was negative, and was but little acted upon. With the strongest mixture, the liquid in contact with the hot plate soon became green.
2. $\frac{1}{8}$	·0016	
3. $\frac{1}{4}$	·0040	
4. $\frac{1}{2}$	·0287	
5. 1	·1234	
6. 2	·2005	

Hydrobromic Acid.

No.	Weak acid diluted to 10 ozs.		Value of Deflection.	
1. $\frac{1}{2}$	·0149	Hot plate negative. Both plates much stained, the cold one the most so.
2. 1	·0647	

Crystallized Boracic Acid.

The hot plate was positive. A series of six solutions was employed, containing from 50 grains to nearly 400 grains in 20 ounces by measure of water, the strongest being a saturated solution. The currents obtained were extremely feeble, and the plates were not tarnished.

Aqueous Hydrofluosilicic Acid.

Value of deflection .1488. The hot plate was negative, and became a little tarnished.

Pure Sulphuric Acid.

No.	Ounces of strong acid diluted to 20 ozs.		Value of Deflection.	
1. $\frac{1}{16}$0077	The hot plate was negative, and the plates were but little tarnished.
2. $\frac{1}{8}$0161	
3. $\frac{1}{4}$0418	
4. $\frac{1}{2}$0878	
5. 11044	
6. 20327	
7. 40037	
8. 80319	

Pure Phosphoric Acid, solid.

No.	Grains of the glacial acid diluted to 20 ozs.		Value of Deflection.	
1. 10000005	The hot plate was positive, and the plates were not visibly tarnished.
2. 20000005	
3. 40000040	
4. 80000060	
5. 160000370	

Chloride of Copper (Basic; solution filtered).

No.	Grains of the salt diluted to 12 oza.		Value of Deflection.	
1. 1000025	Hot copper positive.
2. 5000198	" negative.
3. 10000025	

Much action on both plates, especially the hot one, and basic chloride of copper formed.

Chlorate of Copper.

In a moderately strong solution of this salt, which had been digested with an excess of carbonate of copper and filtered, the hot plate was negative; value of deflection .2997. Both plates were acted upon, but the hot one the most. The liquid had a feebly acid reaction.

Sulphate of Copper.

No.	Grains diluted to 20 oza.		Value of Deflection.	
1. 249.50016	Hot copper negative.
2. 499.00081	Liquid acid.

Chloride of Cobalt.

No.	Ounces diluted to 12 ozs.	Value of Deflection.	
1. $\frac{1}{2}$ oz. saturated solution	·1247	} Hot copper positive. Li- quid acid. No stains, ex- cept slightly at edge of hot liquid.
2. 3 ozs. " "	1·2726	

Protosulphate of Iron.

No.	Grains diluted to 12 ozs.	Value of Deflection.	
1. 200	·0198	Hot copper negative.
2. 800	·0794	No stains on either plate.

Chloride of Manganese.

No.	Grains diluted to 12 ozs.	Value of Deflection.	
1. 100	·0436	} Hot copper positive. No stains. Liquid neutral, or very faintly acid.
2. 1000	1·0995	

Chromic Acid.

No.	Ounces of strong solution diluted to 12 ozs.	Value of Deflection.	
1. $\frac{1}{2}$	·0087	} Hot copper positive. Both plates much acted upon, ap- parently the cold one the most.
2. 2	18·8034	

Chloride of Chromium.

No.	Ounces of strong solution diluted to 10 ozs.	Value of Deflection.	
1. $\frac{1}{2}$	·0293	} Hot copper positive. The plates appeared unaffected. The solution was weakly acid to test-paper.
2. 1	·1319	

Nitrate of Lead.

No.	Grains.	Value of Deflection.	
1. 100 diluted to 12 ozs.	·0009	} Hot plate negative. No stains. Liquid extremely faintly acid.
2. Saturated solution (undiluted)	·0259	

Sulphate of Zinc.

No.	Grains diluted to 20 ozs.	Value of Deflection.	
1. 287	·0080	} Hot copper negative. { Liquid positive. { neutral.
2. 574	·0048	

Sulphate of Magnesium.

No.	Grains.	Value of Deflection.
1.	50 diluted to 20 ozs.	·0001
2.	100 " "	·0002
3.	200 " "	·0006
4.	246 " "	·0100
5.	400 " "	·0036
6.	492 " "	·0208
7.	800 " "	·0130
8.	1000 " "	·0228
9.	2000 " "	·0607
10.	Saturated solution (undiluted)	·0463

Hot. copper positive, and liquid neutral.

Chloride of Calcium.

1400 grains diluted to 20 ounces. Value of deflection ·2935. Hot copper positive. Liquid neutral. Hardly any stain, most on hot plate.

Nitrate of Strontium.

No.	Grains diluted to 12 ozs.	Value of Deflection.
1.	100	·0368
2.	1000	·2321

Hot plate positive. No stain. Liquid neutral.

Chloride of Strontium.

No.	Grains diluted to 20 ozs.	Value of Deflection.
1.	400	·2088
2.	800	·3277
3.	1600	·6671
4.	3200	·6654

Hot copper positive. Liquid neutral. Hardly any chemical action, most on hot plate.

Nitrate of Barium.

No.	Grains.	Value of Deflection.
1.	100 diluted to 20 ozs.	·0016
2.	Saturated solution (undiluted)	·1170

Hot copper positive. No stain. Liquid neutral.

Chloride of Barium.

No.	Grains.	Value of Deflection.
1.	50 diluted to 20 ozs.	·0016
2.	100 " "	·0049
3.	200 " "	·0145
4.	244 " "	·0257
5.	400 " "	·0214
6.	488 " "	·0259
7.	800 " "	·0234
8.	1600 " "	·0802
9.	Saturated solution (undiluted).	·3142

Hot copper positive. Liquid neutral. A little copper was dissolved by the solution.

Nitrate of Sodium.

No.	Grains diluted to 20 ozs.		Value of Deflection.		
1.	85	·0025	Hot copper negative.	{ Liquid neutral.
2.	255	·0328	{ " positive.	
3.	510	·1015		

Chloride of Sodium.

No.	Grains.		Value of Deflection.		
1.	12·5 diluted to 20 ozs.		·0001	{	Hot copper positive. Tarnish at edge of hot liquid; especially with the strongest solutions.
2.	25	" "	·0012		
3.	50	" "	·0081		
4.	75	" "	·0153		
5.	100	" "	·0293		
6.	150	" "	·0512		
7.	200	" "	·0819		
8.	250	" "	·1016		
9.	300	" "	·1160		
10.	400	" "	·1908		
11.	500	" "	·2241		
12.	600	" "	·2708		
13.	800	" "	·3473		
14.	1000	" "	·4884		
15.	2000	" "	·4237		
16.	Saturated solution (undiluted)		·3479		

Iodide of Sodium.

No.	Grains diluted to 12 ozs.		Value of Deflection.		
1.	100	·0100	Hot copper negative.	{ Liquid alkaline.
2.	1000	·0819	" positive.	

Carbonate of Sodium.

No.	Grains diluted to 20 ozs.		Value of Deflection.		
1.	286	...	·0468	Hot copper positive.	
2.	572	·1673	Liquid alkaline.	

Biborate of Sodium.

Six ounces of a saturated solution diluted to 12 ounces. Hot copper was positive ; value of deflection ·0452.

Sulphate of Sodium.

No.	Grains diluted to 20 ozs.		Value of Deflection.		
1.	50	·0001	Hot copper negative.	} Liquid neutral.
2.	100	·0009	} „ positive.	
3.	200	·0016		
4.	1000	·0170		

Phosphate of Sodium.

No.	Grains diluted to 20 oza.	Value of Deflection.	
1.	358	·0382	Hot copper positive.
2.	716	·0648	Liquid alkaline.

Nitrate of Potassium.

No.	Grains.	Value of Deflection.	
1.	50 diluted to 20 oza.	·0025	Hot plate positive. No stains at all on the plates. Solution quite neutral.
2.	100 " "	·0107	
3.	200 " "	·0259	
4.	400 " "	·0647	
5.	800 " "	·1328	
6.	1600 " "	·2997	
7.	Saturated solution (undiluted)	·2852	

Chloride of Potassium.

No.	Grains.	Value of Deflection.	
1.	25 diluted to 20 oza.	·0010	Hot plate positive, and became tarnished at the edge of the liquid, especially with the stronger solutions. Traces of copper were found to have dissolved. The solutions were neutral to test-paper.
2.	50 " "	·0064	
3.	100 " "	·0145	
4.	200 " "	·0442	
5.	300 " "	·0667	
6.	400 " "	·0882	
7.	500 " "	·1239	
8.	600 " "	·1396	
9.	800 " "	·1874	
10.	1000 " "	·2443	
11.	2000 " "	·6371	
12.	Saturated solution (undiluted)	·8439	

Chlorate of Potassium.

No.	Grains diluted to 20 oza.	Value of Deflection.	
1.	122·5	·0054	Hot plate positive, and became tarnished. Solution neutral.
2.	245·0	·0453	

Bromide of Potassium.

No.	Grains diluted to 20 oza.	Value of Deflection.	
1.	100	·0497	Hot plate negative. } No stains.
2.	500	·1080	" positive. } Liquid neutral.
3.	1000	·3150	

Iodide of Potassium.

No.	Grains diluted to 12 oza.	Value of Deflection.	
1.	100	·0259	Hot plate negative. } No stain.
2.	550	·0452	
3.	1000	·0159	

Iodate of Potassium.

No.	Grains.	Value of Deflection.	
1.	100 diluted to 12 ozs.	·0064	} Hot plate negative. Both plates much stained by formation of iodide of copper.
2.	Saturated solution (undiluted)	·0100	

Acid Carbonate of Potassium.

No.	Grains.	Value of Deflection.	
1.	50 diluted to 20 ozs.	·0049	} Hot plate positive. The liquid on evaporation was green with dissolved copper. Liquid alkaline. Hot plate alone much tarnished.
2.	100 " "	·0170	
3.	200 " "	·0497	
4.	400 " "	·0818	
5.	600 " "	·1329	
6.	800 " "	·1978	
7.	1000 " "	·2441	
8.	2000 " "	·4210	
9.	Saturated solution (undiluted)	·5451	

Carbonate of Potassium.

No.	Grains.	Value of Deflection.	
1.	50 diluted to 20 ozs.	·0122	} Hot plate positive.
2.	100 " "	·0382	
3.	200 " "	·1770	
4.	400 " "	·3719	
5.	800 " "	·7521	
6.	1600 " "	2·2400	
7.	2400 " "	3·7708	
8.	3200 " "	·4367	
9.	Saturated solution (undiluted)	·4031	

Acid Sulphate of Potassium.

Saturated solution (undiluted). Value of deflection ·1047. Hot plate negative.

Bichromate of Potassium.

No.	Grains diluted to 20 ozs.	Value of Deflection.	
1.	295	·0785	} Hot metal positive.
2.	590	·1544	

Chrome Alum.

No.	Grains diluted to 20 ozs.	Value of Deflection.	
1.	249·8	·0019	} Hot metal negative. Liquid of acid reaction.
2.	499·6	·0064	

Aqueous Ammonia.

Copper in a mixture of 4 ounces of water and 400 grains of aqueous ammonia at 180° Fahr. was electro-positive to copper in the same mixture at 60° Fahr.

Nitrate of Ammonium.

No.	Grains diluted to 20 ozs.		Value of Deflection.	
1.	80	·0002	Hot plate negative. Acid reaction.
2.	240	·0228	
3.	480	·0590	

Chloride of Ammonium.

No.	Grains.		Value of Deflection.	
1.	25 diluted to 20 ozs.		·0020	Hot copper positive. So- lutions extremely faintly acid. Both plates tarnished by the stronger solution; but the hot one the most so, and a little copper was dissolved.
2.	50 " "		·0029	
3.	100 " "		·0147	
4.	200 " "		·0647	
5.	400 " "		·1583	
6.	800 " "		·5551	
7.	1000 " "		·6744	
8.	1600 " "		·6258	
9.	2000 " "		·7479	
10.	Saturated solution (undiluted)		·1210	

Aqueous Hydrocyanic Acid.

Scheele's strength. The hot plate was feebly positive. Value of deflection ·0006.

Cyanide of Potassium.

No.	Grains diluted to 12 ozs.		Value of Deflection.	
1.	100	·2854	Hot copper positive. Much gas evolved from the hot plate only in the strongest solution.
2.	1000	1·8164	

Ferrocyanide of Potassium.

No.	Grains diluted to 20 ozs.		Value of Deflection.	
1.	500	·0136	Hot plate positive. Li- quid feebly alkaline. Both plates became pink like new copper.
2.	1000	·0045	

Oxalic Acid.

No.	Grains.		Value of Deflection.	
1.	25 diluted to 20 ozs.		·0001	The hot plate was nega- tive, and the plates were not tarnished at all.
2.	50 " "		·0002	
3.	100 " "		·0006	
4.	200 " "		·0016	
5.	400 " "		·0064	
6.	Saturated solution (undiluted)		·0070	

Glacial Acetic Acid.

The hot plate was negative. Seven solutions, containing from $\frac{1}{8}$ ounce to 4 ounces by measure of the acid in 20 ounces by measure, gave only extremely feeble currents. The plates remained bright.

Acetate of Sodium.

No.	Grains diluted to 20 ozs.		Value of Deflection.	
1.	50	·0016	} Hot plate positive. So- lution alkaline to test- paper.
2.	100	·0070	
3.	200	·0198	
4.	400	·0594	
5.	800	·1328	
6.	1600	·2202	
7.	2000	·2850	
8.	2727	·2997	

Acetate of Zinc.

No.	Grains diluted to 20 ozs.		Value of Deflection.	
1.	50	·0001	} Hot plate positive.
2.	100	·0006	
3.	200	·0016	
4.	400	·0025	
5.	500	·0020	
6.	800	·0020	
7.	1000	·0012	
8.	1600	·0004	
9.	2000	·0001	

Crystallized Tartaric Acid.

The hot plate was negative. Eight different solutions, varying in strength from 50 to 3200 grains in 20 ounces by measure of the solution, were tried; but very feeble currents were obtained, and the plates were not tarnished.

Crystallized Citric Acid.

The hot plate was negative. With a series of seven solutions, varying in strength from 50 to 3200 grains in 20 ounces of liquid, more feeble results, even, than those with tartaric acid were obtained, and the plates were not tarnished. Probably, with this substance and with others where the resulting currents were very feeble, more distinct effects would be obtained by employing a galvanometer of much greater electrical resistance.

Several experiments similar to those already described were made with the apparatus shown in fig. 3. The apparatus consists of a glass beaker containing the liquid, and two platinum electrodes—A being a disk of platinum rivetted to a platinum wire enclosed by a glass tube, B, and C a platinum crucible (for receiving the boiling water) with a platinum wire rivetted to it.

Experiment 1.—With a solution of 100 grains of citric acid in 2 ounces of distilled water, the hot platinum cup was negative, the value of the temporary deflection being ·0007.

Experiment 2.—With 100 grains of tartaric acid in 2 ounces of water, the hot cup was negative, value of deflection ·0001.

Experiment 3.—With 100 grains of racemic acid in 2 ounces of water the hot cup was negative, value of deflection $\cdot 00005$.

The negative condition excited in the hot platinum cup in the solutions of citric and tartaric acid agrees with the results obtained with copper in those liquids.

Fig. 3.



Fig. 4.



I have already shown (*Phil. Mag.* 1857, vol. xiii. p. 1) that the currents obtained with platinum electrodes are not due to the influence of atmospheric air upon the liquid and metal at their line of mutual contact; for, in the experiments there recorded, atmospheric air was entirely excluded, and the liquids were previously well boiled.

To test the influence of *size* of the *cold* electrode, I took a platinum dish, A (see fig. 4), 5 inches wide and $1\frac{1}{2}$ inch deep, in a glass vessel of the annexed form, B, closed at its lower end by a cork, and containing in its neck two platinum electrodes, one consisting of a wire, C, and the other of a sheet 2 inches long and 2 inches wide in the form of a cylinder, D.

With a cold mixture composed of $3\frac{1}{2}$ ounces of water and $\frac{1}{4}$ of an ounce by measure of strong sulphuric acid, and the *sheet* of platinum as the lower electrode, on pouring boiling water into the dish a deflection of the value of $\cdot 0064$ was obtained, the cold electrode being positive; but with the *wire* as the lower electrode no perceptible deflection occurred. These results were obtained repeatedly. The electric currents are therefore largely dependent upon the size of the cold electrode.

General Results.

The chief fact brought out conspicuously by these experiments with copper dishes is, that in many cases an increase of chemical action produced by heat, instead of making the hot metal electro-positive, makes it considerably negative.

The results show that hot copper was positive to cold copper in the following liquids:—hydrochloric, hydrocyanic, boracic, and tribasic or orthophosphoric acids; chloride of copper (weak solution); chloride of cobalt; chloride of manganese; chromic acid; chloride of chromium; sulphate of zinc (weak solution); sulphate of magnesia; chloride of calcium; nitrate and chloride of strontium; chloride of barium; nitrate of sodium (strong solution); chloride, iodide, carbonate, and biborate of sodium; sulphate

of sodium (strong solution); tribasic phosphate of sodium; nitrate, chloride, and chlorate of potassium; bromide of potassium (strong solution); iodide of potassium (strong solution); carbonate, acid carbonate, and bichromate of potassium; aqueous ammonia; chloride of ammonium; cyanide and ferrocyanide of potassium; acetate of zinc; and acetate of sodium. And negative in the following ones:—nitric, chloric, hydrobromic, hydrofluosilicic, and sulphuric acids; ferrous sulphate; chloride of copper (strong solution); sulphate of copper; sulphate of zinc (strong solution); nitrate and iodide of sodium (weak solutions); bromide and iodide of potassium (weak solutions); iodate of potassium; chrome alum; nitrate of ammonium; oxalic, acetic, tartaric, and citric acids. The number of liquids in which hot copper was positive was thirty-six, and of those in which it was negative was twenty.

In several instances where the hot metal was negative with a weak solution, it became positive with a strong one—for instance, with sulphate of zinc, nitrate, iodide, and sulphate of sodium, bromide and iodide of potassium; but with chloride of copper the reverse occurred. These results may be connected with the fact that in weak neutral solutions the chemical action is generally the most feeble, and therefore interferes the least with the direct influence of the heat in producing electric currents.

The influence of free hydrochloric, hydrocyanic, boracic, orthophosphoric, and chromic acids was to make the hot copper positive; whilst that of nitric, chloric, hydrobromic, hydrofluosilicic, sulphuric, and some of the organic acids was to make it negative.

In consequence, probably, of the small amount of interference by chemical action in solutions of oxalic, acetic, tartaric, and citric acids, the direct influence of the heat made the copper negative—similar to its influence on platinum in all acid liquids which do not attack that metal.

The nature of the acid in a salt appears to exert much more influence than that of the base on the direction of the current; for instance, in nearly all chlorides, including those of a considerable variety of bases, hot copper was positive, probably because copper is more readily attacked by acids than by bases.

In all decidedly alkaline liquids the hot copper was positive; this is similar to the behaviour of platinum in such solutions, and is probably due to the same cause, viz. the direct influence of the heat, as well as to chemical action.

The results also show that the quantity of the current obtained with any given liquid generally increases with the number of molecules of the substance contained in the solution; in some cases, however, as with sulphuric acid, carbonate of potassium, chloride of ammonium, and acetate of zinc, there was a limit to this increase; and beyond that limit the quantity of the current decreased up to the point of saturation of the liquid.

In the great majority of cases the value of the deflection increased much more rapidly than the strength of the solution, particularly with solutions

of sulphate of magnesia, and also of hydrochloric acid and of chloride of sodium, probably because two causes operated, viz. increased strength of solution and diminished conduction-resistance; in a very few cases, however, the opposite result took place, as with solutions of chloride and nitrate of strontium.

Inversions of the direction of the deflection by difference of strength of the liquid occurred with solutions of chloride of copper, sulphate of zinc, nitrate, iodide, and sulphate of sodium, bromide and iodide of potassium.

Irregularities of the amount of deflection were very apt to take place with liquids which gave strong deflections, or which acted much upon the copper plates (for instance, nitric acid), especially if bubbles of air remained under the plates, or the dishes were wetted on their side above the liquid by the solution.

In certain acid liquids, viz. nitric, chloric, hydrobromic, hydrofluosilicic, and sulphuric acids, the hot copper was strongly negative (notwithstanding the chemical action upon it was distinct, and in some cases even strong); this is similar to the electrical behaviour of platinum in such liquids, and may be attributed either to the more direct influence of the heat alone (such as occurs with platinum plates), or to a different influence of the chemical action produced by the heat. Both these causes probably operate in such cases.

It is probable that in all cases where the hot copper was positive in liquids of strongly acid reaction, the positive condition was due to chemical action alone.

With some liquids, especially with solutions of hydrocyanic, boracic, acetic, tartaric, and citric acids, the deflections were very feeble, and the chemical action on the plates not perceptible; whilst with others, such as nitric and chloric acids, solutions of the chlorides of strontium, sodium, potassium, and ammonium, and of carbonate, acid carbonate, and cyanide of potassium, the deflections were considerable, and the chemical action distinct, and in some cases strong. In none of the liquids (except hydrobromic and chromic acids) did the hot plate appear to be *less* stained or corroded than the cold one; probably in all cases it was the most corroded, although in some cases the corrosion was not perceptible.

The amount of deflection was not always proportionate to the amount of chemical action; for instance, with solutions of chloride of copper and iodate of potassium there was considerable corrosion, but only feeble currents, probably because the plates became covered with a badly conducting film, whilst, with hydrochloric acid, chloride of cobalt, chloride of manganese, and nitrate of potassium the reverse occurred.

I consider the currents in all these experiments of difference of temperature to be due either, 1st, to the direct influence of heat, the effect of which is to make the hot copper negative in acid liquids and positive in alkaline ones (see *Phil. Mag.* 1857, vol. xiii. p. 1); 2nd, to chemical action, which sometimes overpowers the direct influence of heat and reverses the effect;

or, 3rd, to both these influences combined. The more ultimate cause, however, of the phenomena in these cases must be sought for in the *molecular movements* produced by heat in the metals and liquids.

The currents obtained with copper plates were no doubt influenced in their amounts (if not also in their direction) by the oxidizing action of the air upon the liquid and metal at their line of mutual contact; for we know that metals in contact with liquids oxidize much more quickly if oxygen has access to their wet surfaces. And the currents were also influenced by the action of unequal temperature upon this air-contact line; for we know that wet metals oxidize still more rapidly if heat is applied.

Influence of line of contact of liquid and metal with the air.

That the length of line of contact of the liquid and copper with the air is capable of producing electric currents was shown by the following experiments:—

Two strips of sheet copper of the annexed form, fig. 5, $\frac{3}{4}$ inch wide, and

Fig. 5.



Fig. 6.



12 inches long in the longest limb, were cut from contiguous parts of a sheet of copper, and, after being perfectly cleaned, were coiled into the shape represented by the annexed sketch, fig. 6. They were then placed in a flat-bottomed porcelain dish and connected with the galvanometer, one of the spirals being supported at about $\frac{1}{4}$ inch higher than the other by means of a triangle of glass rod. The liquid to be examined was then poured into the dish until it just (and completely) covered the lower spiral, and the direction and amount of the permanent deflection noted. The positions of the spirals were then reversed and the electrical effects again noted.

Experiment 1.—With a liquid composed of 100 grains of cyanide of potassium dissolved in 12 ounces of water, whichever of the spirals was only partly submerged and therefore had the longest air-line, was strongly electro-negative to the wholly submerged one.

Experiment 2.—With a mixture of one measure of strong nitric acid and ten measures of water, deflections of somewhat less amount, but in precisely similar directions to those of experiment 1, took place.

Experiment 3.—With dilute hydrobromic acid the directions of the deflections were also similar, but still less in amount.

Experiment 4.—With a half-saturated solution of borax very feeble de-

flections, agreeing in direction with those of the other experiments, were obtained.

These results show the necessity (which I have already mentioned) of excluding air-bubbles from beneath the copper dishes, and of not wetting the sides of the dishes by the liquid above the level of their immersion.

To ascertain the influence of *difference of temperature* of the air-contact line I soldered two strips of perfectly similar sheet copper, each 12 inches long and $\frac{1}{2}$ inch wide, in the form of circular hoops 4 inches in diameter upon the bottoms of two tin cups, and ground the edges of the strips perfectly level, and soldered copper wires to them for connecting with the galvanometer. Two glass triangles were now put into the apparatus, fig. 1, one in each dish, to support the cups, and a mixture of one measure of nitric acid and 12 measures of distilled water poured in until it just touched the edges all round of the perfectly horizontal copper rims resting on the triangles. After the needles of the galvanometer had settled at zero, about ten ounces of boiling water was poured into one of the cups; a temporary deflection of the value $\cdot 0560$, and a permanent one of value $\cdot 0759$, were produced, the hot metal being negative. The direction of the current in this experiment agrees with that obtained with the same mixture and the copper dishes; and the result indicates that a large proportion of the quantity of the current obtained with copper dishes in dilute nitric acid was due to the action of the air-contact line.

The influence of the air-line is largely *chemical*. "A piece of copper wire *wholly submerged* in the acid [dilute sulphuric] so as to entirely exclude any portion of it coming into contact with the air, has remained for many months without imparting the slightest tinge to the liquid." "But on suffering the liquid to evaporate so as to bring the upper end of the metal near to its surface, the instant the slightest portion becomes exposed chemical action immediately begins."

"Two equal portions of wire were similarly placed in acid, only that one was fully exposed to the atmosphere in an open tube, while the other was placed in a phial, the acid occupying half its height, and was kept closely corked for several weeks—after which the fully exposed metal had lost in weight two-fifths more than the one which had been excluded from contact with fresh portions of air, showing that contact with the atmosphere in bulk is necessary to the fullest action"*.

Experiments with Liquids of unequal strength.

To throw some light upon the questions,—1st, Is the quantity of the current simply a result of the difference of number of molecules of liquid which touch the hot plate compared with those which touch the cold plate? and, 2nd, What amount of difference of strength of a liquid is equal to the amount of difference of temperature employed?—I brought the two

* "On the Theory of the Voltaic Pile," Bridgman, Phil. Mag. Nov. 1869.

copper dishes into contact with liquids of unequal strength instead of unequal temperature.

The tube C (fig. 1) was filled with the stronger mixture and closed at its end in the dish A by an india-rubber bung, and the dish B filled to the line F with the same mixture; the dish A was then filled with the weaker mixture up to the same level and the bung slowly withdrawn. The two copper dishes, previously connected with the galvanometer, were next simultaneously immersed in the mixtures and the effect noted. The following are the results obtained by this method:—

Nitric Acid.

Experiment 1.—In A, 1 volume of acid diluted to 80 volumes. In B, 1 volume diluted to 40. Copper in A was positive temporarily, value $\cdot 0270$; and permanently, value $\cdot 0198$.

Experiment 2.—In A, 1 volume of acid diluted to 40 volumes. In B, 1 volume of acid diluted to 20 volumes. The copper plate in A was first positive temporarily, value of deflection $\cdot 0064$; and then that in B permanently, value $\cdot 2850$.

Experiment 3.—In A, 1 volume of acid diluted to 40 volumes. In B, 1 volume diluted to 10 volumes. Copper plate in B was positive temporarily, value $\cdot 4863$; and permanently, value $\cdot 0819$.

Hydrochloric Acid.

Experiment 1.—In A, 1 volume of acid diluted to 40 volumes. In B, 1 volume diluted to 20 volumes. The copper in B was positive temporarily, value $\cdot 9608$; and permanently, value $\cdot 1087$.

Experiment 2.—In A, 1 volume of acid diluted to 40 volumes. In B, 1 volume diluted to 26·66 volumes. The copper in B was positive temporarily, value $\cdot 3479$; and permanently, value $\cdot 0702$.

Chloric Acid.

In A, 1 volume of acid diluted to 80 volumes. In B, 1 volume diluted to 40 volumes. The copper in B was positive temporarily, value $\cdot 0036$; and permanently, value $\cdot 0009$.

Sulphuric Acid.

In A, 1 volume of acid diluted to 80 volumes. In B, 1 volume diluted to 40 volumes. The copper in B was positive temporarily, value $\cdot 0467$; and permanently, value $\cdot 0330$.

On examining these results, it will be perceived, 1st, that only in one half the number of the experiments did increased strength of liquid produce electrical currents similar in direction to those produced by increased temperature; and therefore the heat does not act simply by causing a greater number of molecules of each individual substance to touch the hot plate; and, 2nd, that only in one of the experiments was the copper in the weaker liquid both temporarily and permanently positive to that in the

stronger; whilst in five of the experiments the copper in the stronger liquid was temporarily and permanently positive to that in the weaker. Increase of strength of the liquid therefore made the copper positive in five cases out of six.

In the fourth experiment with hydrochloric acid with difference of temperature, and in the second one with difference of strength, the mixture in each case consisting of 1 volume of acid diluted to 40 volumes with water, an increase of temperature from 16° to about 98° C. produced a deflection of the value $\cdot 2854$, whilst an increase of strength to 1 volume in 26.66 gave a deflection in the same direction of the value $\cdot 3479$. An increase of temperature of about 82° C. was not quite equal in electrical effect to an increase of 50 per cent. in the number of molecules of the acid which touched the plates.

In the third experiment with chloric acid with difference of temperature, and in the single one made with difference of strength, the mixture in each instance consisting of 1 volume of the acid diluted to 80 volumes with water, an increase of temperature of about 82° C. produced an electrical effect of $\cdot 0040$; whilst an increase of 100 per cent. in the number of molecules of the acid which touched the plates produced an opposite electrical effect of $\cdot 0036$.

In the third experiment with sulphuric acid with difference of temperature, and in the single one made with difference of strength, each being with a mixture of 1 measure of acid in 80 of water, an increase of temperature of about 82° C. caused an electrical effect of $\cdot 0418$, and an increase of 100 per cent. in the number of molecules of acid which touched the plates caused an opposite electrical effect of $\cdot 0467$.

A liquid thermo-electric battery.

Acting upon the general results thus obtained in this subject, I constructed a liquid thermo-electric battery consisting of twelve glass tubes,

Fig. 8.



$\frac{1}{4}$ of an inch in diameter and 10 inches long, closed at one end (and containing a platinum wire hermetically sealed in that end), and bent to the

form shown in fig. 7, each tube being filled with a conducting liquid, and its outer end closed by a cork, in which was fixed a second platinum wire to dip into the liquid.

Fig. 8 represents the apparatus; A A is a wooden stand supporting a tin box, B. The box is water-tight, and has in its lower surface a long semi-circular cavity (shown by dotted lines) to receive the upper ends of the twelve tubes. To the back of the box is fixed a short cylinder of tin, C, closed at its outer end. When the apparatus is in action, the box is filled with hot water, and the water kept boiling by means of a lamp placed beneath the tube C. The twelve tubes were kept in position by divisions of wood fixed to the back of the stand, as shown in the figure.

The tubes 1, 3, 5, 7, 9, and 11 were filled with a previously boiled and cooled mixture of $\frac{1}{4}$ of an ounce of sulphuric acid, and 19 ounces of distilled water; and the others, viz. 2, 4, 6, 8, 10, and 12, with a similarly prepared solution of 110 grains of hydrate of potassium dissolved in 19 ounces of distilled water.

The platinum wires were connected, in the order shown in the sketch, by means of small binding-screws not represented in the figure.

On connecting the terminals with a galvanometer containing about 180 turns of moderately coarse copper wire, and applying heat to the upper electrodes and ends of tubes by means of the boiling water, no deflection of the needles took place; but on substituting a Thomson's reflecting galvanometer, which offered a resistance of 3040.7 B.A. units ($=77872.327$ miles of copper wire $\frac{1}{8}$ of an inch thick), a deflection of 40 degrees was readily obtained, the hot platinum wire in the dilute acid being negative, and that in the alkali positive, as shown by the direction of the arrows in the sketch.

From these results it is evident the quantity of the electric current produced was exceedingly small, and its intensity considerable. By employing electrodes of larger surface, such as spirals of platinum wire and more concentrated liquids, the quantity of the current would be very largely increased. (See Phil. Mag. 1857, vol. xiii. p. 1.)

Fig. 9.



Fig. 9 represents a simpler arrangement of this apparatus, in which only

one kind of liquid, either acid or alkaline, is employed. The electrodes in this arrangement must be disposed in the order represented by the figure.

Influence of Friction.

To ascertain if the *friction* of one of the electrodes against the liquid had similar effects to those produced by the direct application of heat, I employed the apparatus shown in fig. 10. The sketch does not require explanation.

Fig. 10.



Experiment 1.—By immersing two stout copper wires vertically in an acidulated solution of cupric sulphate and rotating one of them at a speed of about 5000 revolutions per minute, the rotating wire became electro-positive.

Experiment 2.—With a saturated solution of borax, the rotating wire was positive.

Experiment 3.—With a solution of cyanide of potassium, the rotating wire was negative.

Experiment 4.—With stout platinum wires in an acidulated solution of cupric sulphate, the rotating wire became negative.

Experiment 5.—With platinum wires in a solution composed of 200 grains of carbonate of potassium in 40 ounces of distilled water, the rotating wire was faintly positive, and similarly in a very dilute solution.

Experiment 6.—With two platinum disks one above the other in a strong solution of carbonate of potassium, revolving the upper disk at a speed of about 5000 revolutions per minute made it electro-positive.

Experiment 7.—With an acidulated solution of cupric sulphate, the revolving disk became feebly negative.

On comparing these results with those obtained by unequal temperature, we find that the directions of the currents in the two classes of cases were reverse with copper in solutions of acidulated cupric sulphate and cyanide of potassium, and similar in a solution of borax; and with platinum in solutions of acidulated cupric sulphate or carbonate of potassium the in-

fluence of friction and of increased temperature upon the direction of the currents were the same. The molecular movements, therefore, produced by friction are not in all cases similar to those produced by heat.

Influence of Magne-optic rotating-power of the Liquids.

Being desirous of determining whether the thermo-electric properties of liquids were dependent on the molecular structure by virtue of which liquids under the influence of magnetism polarize light circularly, I made the following apparatus and experiment:—

A and B (fig. 11) are two straight glass tubes, about $\frac{3}{4}$ inch in diameter and 10 inches long, with two similar (but bent) tubes, C and D, attached to their free ends by india-rubber tubing. The sloping ends of the straight tubes are ground flat, and are joined together securely at their edges by melted shellac, with a thin and projecting sheet of platinum between them to separate the liquids. E and F are two strong electro-helices wound upon stout tubes of soft iron which enclose the glass tubes. The apparatus is secured upon a board in an inclined position with the sloping ends of the tubes uppermost; and the two helices are held together at their upper ends by an india-rubber band, G. I filled one of the tubes with a clear and strong solution of perchloride of iron (of negative magne-optic rotatory power, see Verdet, Phil. Mag., June 1858), and the other with a similar solution of chloride of nickel (of positive magne-optic rotatory power), and connected the liquids in the bent tubes with a galvanometer 16 feet distant by means of the platinum wires H and I. I now excited the helices in various ways by means of 12 strong Grove's cells; no current was induced in the liquid. I next heated the junction of the tubes gradually; the solution of iron became thermo-electro-positive, and a steady but feeble deflection of the needles took place; and during the continuance of this current I again excited the helices in various ways as before; again no electrical effects were produced. The results of this experiment strongly support the conclusion that the thermo-electric properties of liquids are not dependent upon the magne-optic polarizing power of the liquids, nor upon the properties of their mass.

On examining the thermo-electric properties of the solution of ferric chloride with platinum plates in the apparatus described in the 'Philosophical Magazine,' 1857, vol. xiii. p. 1, the hot platinum was strongly negative, value of temporary deflection $\cdot 8475$. With the nickel solution, similarly examined, the hot plate was also negative, value of deflection $\cdot 0409$. These results agree with that obtained with the two tubes in the last experiment, the more positive condition of the iron solution than that of the nickel one determining the direction of the current in that experiment.



General Conclusion.—The electric currents produced by the direct influence of unequal temperature or friction of platinum or copper electrodes, in conducting liquids which do not act chemically upon those metals, have their origin in temporary changes of cohesion of the layers of metal and liquid which are in immediate and mutual contact, and may be considered a very delicate test of the kind and amount of temporary molecular movements produced by those causes.

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Messrs. J. M. Johnson and Co.

March 2, 1871.

General Sir EDWARD SABINE, K.C.B., President, in the Chair.

In accordance with the Statutes, the names of the Candidates for election into the Society were read as follows :—

Andrew Leith Adams, Surgeon-Major.

Robert Dudley Baxter.

William Henry Besant, M.A.

Henry Bowman Brady.

William Budd, M.D.

George William Callender, F.R.C.S.

Edwin Kilwick Calver, Capt. R.N.

William Carruthers.

Frederick Le Gros Clark.

John Cleland, M.D.

Herbert Davies, M.D.

Walter Dickson, M.D.

Henry Dircks.

August Dupré, Ph.D.

Robert Etheridge.

Alexander Fleming, M.D.

Peter Le Neve Foster, M.A.

Wilson Fox, M.D.

Arthur Gamgee, M.D.

Thomas Minchin Goodeve, M.A.

Frederick Guthrie, B.A.

John Herschel, Capt. R.E.

Edmund Thomas Higgins, F.R.C.S.

Rev. Thomas Hincks, B.A.

Trevenan James Holland, Major, C.B.

Charles Horne.

Rev. A. Hume, LL.D.

Edmund Charles Johnson.

M. Kelburne King, M.D.

John Leckenby.

Alexander Moncrieff, Capt., M.A.

Thomas George Moutgonerie, Major R.E.

Richard Norris, M.D.

Edward Latham Ormerod, M.D.

Oliver Pemberton.

John Arthur Phillips.

Richard Quain, M.D.

Edward James Reed, C.B.

George West Royston-Pigott, M.D.

Carl Schorlemmer.

John Shortt, M.D.

Peter Squire.

Edward Thomas.

Edward Burnet Tylor.

Cromwell Fleetwood Varley.

Arthur Viscount Walden, P.Z.S.

A. T. Houghton Waters, M.D.

Charles William Wilson, Capt. R.E.

John Wood, F.R.C.S.

Edward Perceval Wright, M.D.

I. "Further Experiments on the effect of Diet and Exercise on the Elimination of Nitrogen." By E. A. PARKES, M.D., F.R.S.
Received January 28, 1871.

In the Proceedings of the Royal Society (No. 89, 1867, and No. 94, 1867) some experiments were published on the elimination of nitrogen, during exercise and rest, on a nitrogenous and a non-nitrogenous diet. The result of both series was so far to confirm the experiments which show that the changes in the nitrogen of the urine and fæces are small in extent and afford no measure of the work; but there did appear to be a slight effect produced in two ways :—

1. There was an increased, though slight, outflow of nitrogen after work.
2. There was apparently a slight lessening of the outflow during work, not dependent on diminution in the amount of urinary water. In the state of rest also, when the diet was equal, there was no lessening, but a slight excess, in the excretion of nitrogen as compared with a period both of forced and ordinary exercise.

Professor Karl Voit, of Munich, who has a worldwide reputation for his numerous and important contributions to this subject, and who denies that exercise produces any change in the nitrogen, has taken exception to some of these experiments, on the ground more particularly that the daily ingress of nitrogen could not have been kept sufficiently stable. I believe the experiments, showing as they do in two men a remarkable agreement in the amount of nitrogen eliminated, and the fact that the two great articles of diet by which nitrogen enters (meat and bread) were selected and weighed with great care* and from the analyses appeared to be of constant composition, prove that the alterations in the daily inflow of nitrogen must have been very small and less than those of the outflow.

Undoubtedly, however, to insure an absolute uniformity in the entrance of nitrogen in men, is a very difficult matter as long as only ordinary diet is given. The employment of prepared or concentrated food, on the other hand, cannot be considered as good as common diet for such experiments; for the body is unaccustomed to the particular form in which the food is given.

It was determined to repeat the experiments in two or three ways. First, not only to use ordinary diet with the usual attention to keep it as uniform as possible from day to day, but to select hours for rest and exercise when the influence of diet is least perceptible, viz. twelve or fourteen hours after food; then in a second series to use prepared food in which the amount of nitrogen is absolutely constant; and thirdly to use a diet without nitrogen.

Unfortunately the experiments on preserved food failed on account of the health breaking down in a few days and before any exercise could be taken. On the ordinary diet also an unexpected difficulty arose, but still the results are worthy of record. The experiments on the non-nitrogenous diet are confirmatory of the former results, as far as the increased elimination after exercise is concerned.

As the details of the experiments would occupy too much space, I have given only mean numbers when these were sufficient to fairly show the results, and have omitted all details of the chloride of sodium, free acidity of the urine, and other matters.

The subject of the experiments was T. C., a perfectly healthy soldier

* The meat was beefsteak, and was selected free from visible fat, and was always cooked in the same way, i. e. fried. The bread was the hospital bread, made daily with the same flour, water, and salt, baked at the same heat and for the same time, and having the same amount of crust and crumb.

who had never had a day's illness in his life. He was 25 years of age, weighed usually 145 lbs., and is of very temperate habits. He had been an iron-worker before enlistment, and is an extremely powerful man; the girth of the chest was $37\frac{3}{4}$ inches.

FIRST SERIES OF EXPERIMENTS.

Ordinary regulated diet.

During 20 days the man received daily beefsteak weighing 14 ounces when raw, 1 ounce of fat for cooking, 16 ounces of bread, 1 ounce of butter, 6 ounces of milk, 16 ounces of potatoes, $1\frac{1}{2}$ ounce of sugar, 36 fluid ounces of infusions of tea and coffee, and 16 ounces of water. The amount of nitrogen was determined at 300 grains; it might be a little more or less, but still from day to day its amount was the same as far as it could possibly be kept so. This diet was selected in this, as in the former experiments, because it is the usual ration of the Army Hospital Corps to which this man belonged, and therefore there was no fear of a change in the food itself producing any effect.

He took his meals always at the same time; viz. breakfast at 10 A.M., dinner at 3 P.M. (when he took the whole of his meat), and tea at $\frac{1}{4}$ to 6. After tea he took 6 ounces of water at 10 P.M., but no solid food.

The urine was collected from 10 A.M. to 6 A.M. on the following morning; then from 6 A.M. to 8 A.M. and from 8 A.M. to 10 A.M. As all food was taken between 10 A.M. and 6 P.M., it was expected that the urine from 6 A.M. to 10 A.M. (viz. from the 13th to the 16th hours after food) would be of tolerably constant composition; at any rate there would be less chance of error from the effect of food. In this way, and by keeping as far as possible an equal daily diet, it was hoped to lessen or remove the chances of fallacy from varying ingress of nitrogen. An unexpected circumstance partly disconcerted this hope.

It was anticipated that the amount of urine passing in the two hours from 8 to 10 A.M. would be less than in the two hours from 6 to 8, as being further removed from the time when fluid was taken. But the result was otherwise; there was always more urine passed from 8 to 10 A.M. than in the previous two hours. When this was first noted, it was supposed that an error in collecting the urine had been made; but day after day the result was the same. It seemed to be owing to the influence of sleep and wakefulness. From 6 to 8 the man slept, but from 8 to 10 he was not only awake, but his mind was active, and he talked to two men who worked in the room where he slept; and though his body was kept as quiet as during the previous two hours, the mental condition seemed to cause an increased passage of urine; at least there seemed nothing else to account for the fact that on every day during ten days while he was still in bed, there was more urine passed from 8 to 10 than from 6 to 8 A.M., although no water had been taken except at 10 the night before. The result was

that the urea did not fall as was expected, though its percentage was lessened.

Liebig's mercuric nitrate solution (the chloride of sodium being got rid of and the usual correction for dilution being made) and Voit's plan for the determination of nitrogen by soda-lime were both used, so as to afford a control of the observations.

The daily weight of the body, the temperature of the axilla and rectum, the pulse, the weight of the stools, &c. were also determined. Almost all the experiments were repeated, and several were performed three and four times*.

During the first ten days he remained in bed from 10 P.M. to 10 A.M., taking during the day his ordinary exercise. During the second 10 days he went to bed at 10 P.M. as before, got up at 6 A.M., worked for two hours, and then went to bed again at 8 A.M. until 10 A.M. The work consisted in dragging a cart weighing 710 lb. 4 miles in two hours. Supposing the coefficient of traction to be the same as in walking, the amount of work calculated by Haughton's formula would be equal to about 100 tons lifted one foot. His weight at the commencement of the experiment was 146 lb. 2 oz.; it fell regularly during 10 days to 144 lb. 10 oz., viz. 1 lb. 8 oz. During the second 10 days it fell to 142 lb. 12 oz., a loss of 1 lb. 14 oz.

The following are the mean results in the first and second sections of 10 days.

Mean amount of urine, in cub. centims.

	6 A.M. to 8 A.M.	8 A.M. to 10 A.M.	10 A.M. to 6 A.M.	Total.
1st section of 10 days	71·65	100·4	1077·45	1249·5
2nd section of 10 days.....	75·6	85·8	1068·8	1229·4

There was a slight decrease in the urinary water in the two hours of rest following the two hours' exercise.

Mean amount in grammes of mercuric nitrate precipitate taken as urea.

	6 to 8.	8 to 10.	10 to 6.	Total.
1st section of 10 days, } usual exercise†	3·056	2·902	35·189	41·147
2nd section of 10 days, } 2 hours additional exercise from 6 to 8 }	3·142	2·974	36·017	42·133

* Count Wollowicz commenced these experiments with me, but was obliged to discontinue them on account of the illness which eventually proved fatal. Serjeant Turner, of the Army Hospital Corps, very carefully took most of the observations on the temperature and the pulse.

† One day's urea from 6 to 10 A.M. is omitted as the urine was lost.

The results show no diminution in the urea during the two hours of exercise; the slight increase may perhaps be disregarded. In the two hours of rest, there is equally inconsiderable increase after exercise; the excess in the 4 hours of the second section of 10 days was 1.58 gramme. In the next 20 hours there was an increase of .828 gramme, equal to .3864 gramme, or 6 grains of nitrogen. The differences are so small as probably to fall within the limit of error, and it is impossible to affirm that exercise to the amount of 100-foot tons in two hours made any alteration in the urea.

Mean amount of nitrogen as determined by soda-lime.

	6 to 8.	8 to 10.	10 to 6.	Total.
1st section of 10 days*: rest from 6 to 10 A.M..... }	1.489	1.492	16.902	19.883
2nd section of 10 days: exercise from 6 to 8 A.M., rest from 8 to 10 A.M. ... }	1.438	1.399	16.924	19.761

The nitrogen by soda-lime showed very slight variations. There was a slight decrease in the 2 hours' exercise and in the 2 hours' rest after exercise over the corresponding period; but it is so trifling that I hesitate to draw any conclusions. The figures show that the slight increase in the urea was, as supposed, an unavoidable error. Looking to the figures of the nitrogen by soda-lime as more correct, it seems that 2 hours' additional exercise produced no marked change in the outflow when the inflow of nitrogen was constant. This result, as far as it goes, certainly bears out Voit's assertion of the constancy of the nitrogen, but it does not destroy the conclusions formerly drawn that the nitrogen lessens during exercise and increases afterwards, because the amount of exercise was in the present case very much less; and as the alteration in the nitrogen even in the former experiments, with much more severe exercise, fell within narrow limits, it might easily have been anticipated that work of only one-sixth the amount would be inappreciable. The method of experimenting also does not appear to me to be so good as that formerly used.

Amount of nitrogen, in grammes, in the stools.

	Percentage composition.			Amount in 24 hours.		
	Solids.	Water.	Nitrogen.	Solids.	Water.	Nitrogen.
8th day of 1st period..	21.592	78.048	1.166	42.929	152.691	2.279
7th day of 2nd period.	25.00	75.00	1.379	26.115	78.345	1.441

* The nitrogen in the 2 first days is not included in the mean of the hours 6 to 10, as the urine was lost on one occasion, and on the other the experiment was uncertain. The mean of these hours (6 to 10) is therefore for 8 days.

On the 8th day there was a large stool, showing previous accumulation. Taking the weight of all the stools during the two sections of 10 days, and using the percentage composition of nitrogen of the one day in each series, the mean daily excretion of nitrogen was 1·807 gramme in the 1st section, and 1·766 gramme in the 2nd section of 10 days.

The pulse and the temperature of the axilla were taken every two hours from 6 A.M. to 10 P.M. The temperature is in Fahr. degrees:—

Mean pulse and temperature.
First section of 10 days.

	Hours.									Mean of day
	6.	8.	10.	12.	2.	4.	6.	8.	10.	
Pulse	64·6	66·3	67·8	79·3	65·5	75	74·6	74·2	69·5	70·65
Temp. of axilla	97·94	98·04	98·32	98·62	98·32	98·48	98·49	98·52	98·41	98·32
Temp. of rectum	99·1	99·2	99·27	99·48	99·26

Second section of 10 days.
(Additional exercise from 6 to 8 o'clock.)

	Hours.									Mean of day.
	6.	8.	10.	12.	2.	4.	6.	8.	10.	
Pulse	62·4	82·2	68·4	76	68	74·4	69	69·9	64·6	70·42
Temp. of axilla	98·24	98·24	98·33	98·23	98·41	98·45	98·42	98·44	98·38	98·34
Temp. of rectum	99·4	99·2	99·22	99·22	99·26

The effect on the pulse of the exercise from 6 to 8 in the 2nd period was interesting. The exercise brought up the pulse 16 beats at 8 o'clock over the corresponding period ; but the pulse afterwards fell below the beats of the 1st period, and the result was a perfect balance of the day's work, so that the mean pulse of the day was the same in both periods. This shows how completely the heart, if called on for exertion, compensates itself by subsequent rest. The exercise made little difference in the temperature of axilla or rectum ; there was a slight rise at 8 A.M. corresponding with the pulse after exercise in both axilla and rectum, but the mean of the day was the same.

SECOND SERIES.
Prepared concentrated food.

It was proposed to give prepared food of uniform composition, so that the daily ingress of nitrogen would be absolutely constant. I was unable to obtain the "pea-sausage" which the German troops are now using in the Field, and used instead a concentrated food which had been sent by an inventor to Sir Galbraith Logan, and by him sent to Netley for report.

It was composed of bread, meat, potatoes, sugar, spices and salt dried together, &c., and was stated to contain everything necessary for nutrition, so that the troops on service would need no other food. It contained 13·65 per cent. of water and 2·73035 per cent. of nitrogen. After preliminary trials to know how much would satisfy hunger, 14 ounces were given daily, containing 10·838 grammes of nitrogen. The food, however, produced such derangement in nutrition (indigestion, heartburn, and headache) that after a few days the experiments were discontinued. In spite of the constant ingress, the elimination of nitrogen varied greatly from day to day, the extreme range being from 7·641 to 15·024 grammes; and the man felt so ill that he begged to discontinue the experiment. It was interesting to note that, in spite of the daily variations in the nitrogen, there passed out in the 5 days nearly the same quantity as entered, viz. 54·920 grammes of exit, as against 54·19 grammes of entrance. He lost weight during the trial. The experiment must be therefore repeated on some future occasion with other prepared food.

THIRD SERIES.

Non-nitrogenous food.

In the experiments formerly related in the 'Proceedings' two men were kept for 2 days at a time without nitrogen. As it seemed to do no harm, the present experiments were now prolonged over 5 days on two occasions. The first was after the man had been well fed with nitrogen, the second after the body had become poor in nitrogen from the restricted supply given in the concentrated food. The non-nitrogenous food consisted of arrowroot, butter (deprived of casein), and lump sugar. Infusion of tea without milk was allowed, but this contained in the day only $\frac{1}{4}$ grain of nitrogen. Hunger was perfectly satisfied by this food; the man felt quite well and could have continued it. The heartburn produced by the concentrated food was at once relieved by this starch and fat diet.

First Experiments on Non-nitrogenous Food.

Previous daily entry of nitrogen = 19.5 grammes.

On the first day of non-nitrogenous food he took his ordinary exercise; on the 2nd took additional exercise, which consisted in digging up potatoes over 576 square feet, lifting the weight (16 stone) into a barrow, and wheeling them home for $\frac{1}{2}$ a mile. On the 3rd day he rested, on the 4th repeated the exercise, on the 5th rested. He did the 4th day's work even better than the 2nd, and could have worked on the 5th day.

The amount of work done cannot easily be calculated; it was a good but not an excessive day's work.

The weight on the first day was 142 lb. 7 oz., and on the last 141 lb. 10 oz. He took daily 60 fluid ounces of water (= 1704 cub. centims.), and as much arrowroot, oil of butter, and sugar as he liked.

Urea, in grammes.

	8 A.M. to 8 P.M.	8 P.M.
1st day, usual work	16·768	8·
2nd day, exercise	7·986	4·
3rd day, rest	3·042	3·
4th day, exercise	3·652	2·
5th day, rest	4·290	3·

Nitrogen.

	Nitrogen by soda-lime.	
	8 A.M. to 8 P.M.	8 P.M. to 8 A.M.
1st day, usual work	8·287	4·304
2nd day, exercise	3·782	2·381
3rd day, rest	1·692	1·396
4th day, exercise	1·764	·9196
5th day, rest	2·052	1·333

The effects of exercise and rest are complicated, increasing elimination dependent on the supply consequently nothing can be concluded from the 2nd day (exercise). On comparing the 3rd day (rest) with the 4th day (exercise), the nitrogen by soda-lime shows a decrease, but as the ureal nitrogen is almost precisely the same, the difference is due to the difference in the elimination of the nitrogenous waste.

lowest point. The kind of exercise, too, viz. digging, which is often attended with intervals of rest, was not a good choice. Moreover, on the 3rd and 4th days he took some common salt, which might possibly have interfered, as it is supposed that chloride of sodium augments the outflow of urea.

The experiment was therefore repeated at the period when the body was poor in nitrogen from the use of the concentrated food.

Second Experiments on Non-nitrogenous Food (Arrowroot, Oil of Butter, and Sugar).

Previous daily entry of nitrogen=10·838 grammes.

In this series the man did his ordinary work, which was not severe, and tolerably uniform, during the first 3 days. Then on the fourth day he marched 32 miles on level ground, carrying the new valise equipment, the service-kit, 40 rounds of ball ammunition, rifle, bayonet, and great coat. In all the weight was 43½ lb., his own weight being 145½ lb. The work done was therefore 712·8 tons lifted a foot, or, in other words, it was an extremely hard day's work. He did 26 miles quite easily, but then was greatly fatigued and got very tender about the feet, and had pains in the calves of the legs. The next day he was, however, quite well, and declared that he did not feel in the least weakened from having been 5 days on starch and butter. The march commenced at 8 A.M., and with intervals of 1½ hour for meals, lasted till 7.30, so that actual muscular work both of arms and legs was going on for 10 hours. The daily ingress of chloride of sodium was uniform during the 5 days.

Elimination of urinary water, in cub. centims.

	8 A.M. to 8 P.M.	8 P.M. to 8 A.M.	Total.
1st day, usual work	950	570	1520
2nd day, usual work.....	920	550	1470
3rd day, usual work.....	800	470	1270
4th day, marching	585	325	910
5th day, rest	765	495	1260

Urea, in grammes.

	8 A.M. to 8 P.M.	8 P.M. to 8 A.M.	Total.
1st day, usual work	13·072
2nd day, usual work.....	10·731
3rd day, usual work	9·271
4th day, marching	4·563	2·762	7·323
5th day, rest	9·562	6·435	15·997

Nitrogen by soda-lime.

	8 A.M. to 8 P.M.	8 P.M. to 8 A.M.	Total in 24 hours.	Total nitrogen calculated from urea.
1st day, usual work	5.936	6.100
2nd day, usual work.....	5.427	5.008
3rd day, usual work.....	4.328	4.327
4th day, marching	2.451	1.361	3.812	3.418
5th day, rest	4.997	3.268	8.265	7.465

The effect of the previous small entry of nitrogenous food is clearly seen ; on the 1st day the nitrogen fell almost to the amount of the 2nd day in the previous experiments. On the 3rd day, on the contrary, it was greater than on the corresponding day of the former series.

The amount of nitrogen was actually greater on the 5th day than on the 1st. Except the excessive exercise of the 4th day, no other obvious cause existed for this elimination on the 5th day. No mistake seems possible ; for the urinary water on the 5th day was less in quantity than on the 1st, 2nd, and 3rd days, while the nitrogen was 2 grammes more than even on the 1st day after nitrogenous food was left off. An error in analysis is not possible, since not only were the analyses repeated, but the process by urea gave results corresponding to that by soda-lime. No constitutional condition which could cause excess in elimination was indicated either by the pulse or body temperature, and the man felt perfectly well. I need hardly say that no nitrogenous food was taken ; for it is quite certain that it was not.

The increase in the 5th day in the 1st series, though less marked, is still unequivocal, and there seems therefore no rashness in stating that the conclusion of the experiments formerly laid before the Society is affirmed, viz. that severe exercise causes an increase in the elimination of nitrogen in the period of rest after the exercise. It is noticeable that in this man the increased elimination was not in the hours immediately succeeding, but on the following day, and lasted for some time.

Whether during the period of exercise the nitrogen was lessened is not so clear, as the fall from 4.328 grammes on the 3rd day to 3.812 on the 4th or exercise day might be merely the continuing effect of the deprivation of nitrogen. The experiments formerly recorded seem to me better adapted to determine this point, which, however, certainly requires more evidence in confirmation before it can be accepted.

That changes go on in the muscles during exercise which lead to an increase in the outflow of nitrogen afterwards must, I think, be admitted ; and on this point it seems that the statement of Liebig must be supported against Voit.

It may be interesting to give the mean pulse and temperature during these days of non-nitrogenous feeding for comparison with the normal.

During the day of exercise, however, the observations at 10 A.M. and 2 P.M. on the pulse and all the temperature observations on the marching day were lost, except at 8 A.M. and 8 P.M.

On a diet without nitrogen.

Mean pulse.

	Hours.							Mean of day.
	8 A.M.	10 A.M.	12 NOON.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	
1st 5 days ...	64·4	69·6	69·2	73·6	70·2	75·8	72·6	70·77
2nd 5 days...	68	70·25	75·2	75	74	75·6	72·6	72·95

Mean temperature of axilla.

	Hours.								Mean by day.
	6 A.M.	8 A.M.	10 A.M.	12 NOON.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	
1st 5 days ...	98·08	98·16	98·36	98·36	98·32	98·32	98·36	98·36	98·29
2nd 5 days...	98·08	98·15	98·25	98·2	98·15	98·5	98·15	98·16

Mean temperature of rectum.

	Hours.							Mean of day.
	8 A.M.	10 A.M.	12 NOON.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	
1st 5 days ...	99·0	99·2	99·4	99·4	99·25
2nd 5 days...	98·7	99·04	99·24	99·48	99·115

The mean pulse on the 5th day of the 1st series of non-nitrogenous food was 69·3, and 67·9 on the 5th day of the 2nd series. Both these were rest days, when the heart would naturally beat rather less. Non-nitrogenous diet does not, therefore, materially affect the number of beats of the pulse ; but it decidedly influenced its volume, rendering it smaller, far softer, and more compressible when felt with the finger, and it gave a feeble sphygmographic tracing.

The sphygmographic tracings show this clearly. Ten tracings were taken at 6 A.M. on successive days, when the man was on nitrogenous food, 15 hours after dinner and 12 after the last food. They represent, therefore, the tracings of inanition. Five tracings were taken on successive days at 6 A.M. on the non-nitrogenous food. The same instrument and an equal pressure were always used. The height to which the lever was raised (all conditions of pressure &c. being equal) will therefore show the expansion of the artery. The tracings were carefully measured, and the following is the result:—

			Height of upright line in the sphygmographic tracing at 6 A.M., in inches.
1st day, nitrogenous food, pulse 12 hours after food			0·1375
2nd	„	„	0·1625
3rd	„	„	0·1437
4th	„	„	0·2000
5th	„	„	0·1262
6th	„	„	0·1187
7th	„	„	0·1500
8th	„	„	0·1125
9th	„	„	0·2125
10th	„	„	0·1375
Mean.....			0·1501
1st day, non-nitrogenous food			0·0750
2nd	„	„	0·0810
3rd	„	„	0·1250
4th	„	„	0·0761
5th	„	„	0·0625
Mean.....			0·0839

With an equal pressure the lever was thrown almost double the height when the man was on nitrogenous food. This feebleness of expansion shown by the sphygmograph was quite in accordance with the impression given to the finger. The softness of the pulse proved it was not owing to increased resistance of the arterial wall.

With regard to the temperature, the means are so close to those of the days on ordinary diet, that having regard to the fact that the period was shorter and therefore more liable to error, and that some observations were omitted on the marching-day, it may be concluded that a non-nitrogenous diet continued for 5 days neither raised nor lowered the temperature of the axilla and rectum.

It therefore appears that when the nitrogenous food of a healthy man was reduced to one half for 5 days, and he was then kept for 5 days more without nitrogen, he was able on the 4th day after such deprivation to do a very hard day's work. The non-nitrogenous diet, consisting of butter oil, starch, and sugar, kept him perfectly well; all functions seemed natural, the temperature of the body was unaltered, the pulse became very soft, and the sphygmographic tracings showed very feeble markings; but it was not materially altered in frequency. The circulation appeared to be properly carried on, as far as could be judged of by the man's own feelings. The health, as judged of by the man's feelings and the absence of objective signs, was perfect. On account, however, of the feebleness of the heart's

action it was not thought right to continue the experiments, which had, I believe, sufficiently proved that force necessary for great muscular work can be obtained by the muscles from fat and starch, though changes in the nitrogenous constituents of the muscles also go on which have as one effect an increased though not excessive elimination of nitrogen after the cessation of the work.

II. "Magnetic Observations made during a Voyage to the North of Europe and the Coasts of the Arctic Sea in the Summer of 1870." By Capt. IVAN BELAVENETZ, I.R.N., Director of the Imperial Magnetic Observatory, Cronstadt. In a Letter to ARCHIBALD SMITH, M.A., LL.D., F.R.S. Communicated by Mr. SMITH. Received February 4, 1871.

DEAR FRIEND,—Last summer I made a very interesting magnetic voyage, being invited by Vice-Admiral Possiet to take part in the Arctic Expedition with the Grand Duke Alexis, Lieutenant of the Navy.

The first part of the voyage, from St. Petersburg to Arkangeslsk (1179 miles), by rivers and lakes, I made in a little screw cutter, 27½ feet long, 7½ feet wide, and 2½ feet deep, belonging to the corvette 'Variage;' the second part of the voyage (1716 miles) in the schooner 'Sextant.'

I visited the White Sea and the coasts of the Arctic Ocean; the end of the voyage was in the clipper 'Jemchug' (2461 miles), from Norway to Cronstadt.

On the way I made magnetic observations, the result of which I inclose in this letter. I will ask you to make them known to General Sabine and to the Royal Society.

The observations were made by a small compass which has the edge needle, and which is able to turn from one side to the other. Each observation, the data of which are given, was made in different directions of the instrument, turning the instrument on 120° in azimuth, by which the eccentric errors were taken off. In each direction the needle was turned on both sides for correcting the error of the magnetic direction in the needle. Four observations were made for each position of the needle. By this mode of observation the error does not exceed more than $\pm 1'$.

In the declination table are given the day and the hour of each observation in order to judge of the daily disturbances of magnetism. It would be very interesting to compare these observations with those made at the same time by photography at Kew, and thereby to deduce the magnetic disturbance due to the change of magnetic latitude.

The inclination was observed by a Kew Inclinator belonging to the Compass Observatory, made in London in 1865, and examined by Mr. Balfour Stewart. No doubt it is the most useful instrument for this kind of observation.

The horizontal force was observed by "Captain Belavenetz's Instrument

No.	PLACE OF OBSERVATION.	DECLINATION.				M. and
		Month and Day.	Hour of Observation.	Observed Declination.	Mean Declination.	
1.	SHLISSELBURG (town). Lat. 59° 56' 38" N. Long. 31° 1' 59" E. from Greenwich.	h m h m	o .	a .	31 12.
2.	LODEJNSK POLE (town). Lat. 60° 44' 11" N. Long. 33° 33' 8" E.	3 15 June	5 0 to 5 30 p.m.	1 22.7 E.	1 22.7 E.	3 15 J
3.	VITEGRA (town). Lat. 61° 0' 23" N. Long. 36° 27' 1" E.	7 19 June 7 19 June 8 20 June 8 20 June 7 19 June 7 19 June	8 30 to 9 0 a.m. 9 0 " 9 30 " 9 0 " 9 30 " 9 30 " 10 0 " 6 30 " 7 0 p.m. 7 0 " 7 30 "	2 14.5 E. 2 13.4 2 19.9 2 11.8 2 14.8 2 15.9 E.	2 15.1 E.	7 19 J 7 19 J
4.	VOLOGDA (town). Lat. 59° 13' 30" N. Long. 39° 52' 59" E.	15 27 June " " " " " " " " " " 16 23 " " " " "	10 0 to 10 30 a.m. 10 30 " 11 0 " 11 0 " 11 30 " 11 30 " 12 0 noon 4 0 " 4 30 p.m. 4 30 " 5 0 " 7 0 " 7 30 a.m. 8 0 " 8 30 " 9 0 " 10 0 "	3 17.6 E. 3 22.0 3 24.4 3 23.4 3 33.0 3 26.9 3 25.6 3 25.9 3 24.5 E.	3 24.8 E.	14 28 Ju " " 16 28 "
5.	VELIKY OOSTRO (town). Lat. 60° 45' 45" N. Long. 40° 17' 59" E.	10 June 1 July 21 June 3 July 19 June 1 July 21 June 3 July	5 0 to 5 45 p.m. 7 15 " 8 0 a.m. 6 0 " 6 40 p.m. 8 0 " 8 45 a.m.	6 55.8 E. 7 03.8 7 17.0 7 17.7 E.	7 8.8 E.	19 Ju 1 Ju " "
6.	PIANDA (village). Lat. 62° 56' 11" N. Long. 42° 35' 8" E.	24 June 6 July	4 15 to 5 0 a.m.	4 57.2 E.	4 57.2 E.	24 Ju 6 J

No.	PLACE OF OBSERVATION.	DECLINATION.				M _{and}
		Month and Day.	Hour of Observation.	Observed Declination.	Mean Declination.	
7.	ARHANGELSK (town). Lat. 64° 32' 10" N. Long. 40° 31' 6" E.	26 June	h m h m	° ' ,	° ' ,	
		8 July	11 0 to 11 40 a.m.	5 53.5 E.		29 J
		27	7 15 " 8 10 "	5 49.8	5 44.7 E.	7 J
		9 "				29
		28	9 30 " 10 15 p.m.	5 30.9 E.		11
8.	SOLOVETSKOY (Monastery on an island). Lat. 65° 1' 22" N. Long. 35° 44' 44" E.	4 July	7 0 to 8 0 a.m.	2 49.1 E.		
		16 "	11 15 " 12 15 noon	2 26.5		
		" "	2 45 " 3 45 p.m.	2 20.4		
		" "	7 0 " 7 30 "	2 27.1		7 J
		5				16 J
		17 "	6 50 " 7 30 a.m.	2 39.6		"
		" "	3 20 " 4 0 p.m.	2 15.7		5
		" "	7 40 " 8 16 "	2 24.4	2 28.0 E.	17
		6				"
		18 "	9 30 " 10 15 a.m.	2 38.5		7
		" "	7 20 " 8 0 p.m.	2 20.9		19
		7				"
		19 "	7 0 " 8 0 a.m.	2 38.0		
		" "	11 40 " 12 10 noon	2 24.6		
		" "	3 20 " 4 0 p.m.	2 12.4		
		" "	6 0 " 6 45 "	2 27.2 E.		
9.	CAP. KANINE NOS. Lat. 68° 39' 12" N. Long. 43° 32' 0" E.	10 July	4 20 to 5 15 p.m.	8 5.8 E.		11 J
		22 "			8 4.1 E.	23 J
		11	10 30 " 11 15 a.m.	8 2.3 E.		"
10.	SVIATOY NOS (Cap.). Lat. 68° 9' 50" N. Long. 39° 47' 50" E.	14 July	11 20 to 12 0 noon	4 8.3 E.	4 8.3 E.	14 J
		26 "				26 J
11.	BESIMENNOY (island). Lat. 68° 4' 4" N. Long. 39° 34' 46" E.	13 July	11 0 to 12 0 noon	5 25.7 E.		
		25 "	5 0 " 5 45 p.m.	5 35.2		
		14				13 J
		26 "	8 0 " 8 45 "	5 40.8		26 J
		15			5 41.1 E.	
		27 "	8 30 " 8 50 a.m.	5 52.4		"
		" "	12 30 " 1 15 p.m.	5 45.3		
		" "	3 40 " 4 10 "	5 47.6 E.		

1871.]

made during an Arctic Voyage.

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INCLINATION.			Month.	Day.	Hour of Observation.	Horizontal Force.	Vertical Force.	Total Force.	Force in British Units.
Hour of Observation.	Observed Inclination.	Mean Inclination.							
h m	° ' "	° ' "			h m				
to 2 10 p.m.	73 55.1	73 50.7	June July	28 10	8 0 a.m.	1.4283	4.9307	5.1334	11.13
" 2 10 "	73 46.3								
to 9 15 a.m.	73 41.4	73 45.1	July	7 16 7 19	10 30 a.m. 11 15 a.m.	1.4203	4.8057	5.0003	11.06
" 10 0 "	73 44.8								
" 6 45 p.m.	73 48.6								
" 7 30 "	73 41.4								
" 10 0 a.m.	73 47.3								
" 10 30 "	73 47.0								
to 8 15 a.m.	76 7.2	76 8.4	July	11 23	9 45 a.m.	1.2584	5.1002	5.2532	11.30
" 9 0 "	76 9.5								
to 1 0 p.m.	75 48.5	75 47.6							
" 1 40 "	75 46.6								
to 10 15 a.m.	75 50.2	75 53.5	July	13 25 15 27	12 15 p.m. 12 0 p.m.	1.2653	5.0343	5.1908	11.26
" 10 50 "	75 56.8								

No.	PLACE OF OBSERVATION.	DECLINATION.				
		Month and Day.	Hour of Observation.	Observed Declina- tion.	Mean Declina- tion.	
12.	TERIBERSKAJA GOONA (gulf). Lat. 69° 10' 50" N. Long. 35° 9' 45" E.	17 29 July	h m h m 8 30 to 9 15 a.m.	1 58.1 E.	1 58.6 E.	17 29
		" "	12 30 " 1 15 p.m.	1 52.3		"
		" "	3 15 " 3 45 "	2 3.9		"
		" "	5 30 " 6 15 "	1 50.4		"
		" "	7 30 " 8 15 "	1 50.3 E.		"
13.	KOLA (town). Lat. 68° 52' 48" N. Long. 35° 0' 17" E.	19 31 July	8 30 to 9 15 a.m.	1 30.3 E.	1 21.2 E.	19 31
		" "	11 10 " 11 50 "	1 21.0		"
		" "	3 0 " 3 40 p.m.	1 18.1		"
		" "	5 0 " 5 45 "	1 26.8		"
		" "	8 0 " 8 30 "	1 24.7		"
		20 July	8 30 " 9 10 a.m.	1 34.7		20
		1 Aug.	11 30 " 12 10 p.m.	1 24.6		1
		" "	3 30 " 4 10 "	1 7.0		"
		" "	5 40 " 6 10 "	1 5.1 E.		"
14.	VARD'OR (town in Norway). Lat. 70° 22' 16" N. Long. 31° 6' 51" E.	24 July	3 45 to 4 15 p.m.	0 23.4 W.	0 25.3 W.	25 6
		5 Aug	9 15 " 9 45 a.m.	0 8.5		
		25 July	11 30 " 12 0 noon	0 20.7		
		6 Aug	12 50 " 1 20 p.m.	0 27.5		
		" "	3 10 " 3 40 "	0 22.2		
		" "	5 15 " 5 45 "	0 35.0		
		" "	7 10 " 7 40 "	0 40.0 W.		
15.	HAMMERFEST (town). Lat. 70° 40' 11" N. Long. 23° 42' 0" E.	30 July	1 0 to 1 30 p.m.	6 1.2 W.	6 6.0 W.	30
		11 Aug	3 30 " 4 10 "	6 11.0 W.		11
16.	TROMS'OR (town). Lat. 69° 56' 3" N. Long. 18° 8' 0" E.	1 13 Aug.	9 15 to 9 45 a.m.	10 54.6 W.	11 5.7 W.	1 13
		" "	11 30 " 12 0 noon	11 10.9		"
		" "	1 0 " 1 30 p.m.	11 8.4		"
		" "	3 0 " 3 30 "	11 3.6		"
		2 17 Aug.	9 15 " 10 0 a.m.	11 0.0 W.		"

INCLINATION.			Month.	Day.	Hour of Observation.	Horizontal Force.	Vertical Force.	Total Force.	Force in British Units.
Hour of Observation.	Observed Inclination.	Mean Inclination.							
h m	° '	° '			h m				
to 10 45 a.m.	76 23·4	76 20·2	July	17 20	12 15 p.m.	1·2177	5·0001	5·1550	11·18
" 11 30 "	76 25·2								
" 5 0 p.m.	76 17·7								
" 5 30 "	76 19·4								
to 10 10 a.m.	75 40·0	75 40·1	July July Aug.	19 31 20 1	1 30 p.m. 12 30 p.m.	1·2611	4·9361	5·0946	11·05
" 11 0 "	75 40·0								
" 6 45 p.m.	75 40·3								
" 7 15 "	75 38·3								
" 10 10 a.m.	75 39·5								
" 10 40 "	75 42·4								
to 10 45 a.m.	76 43·1	76 44·8	July Aug.	25 6	12 15 p.m.	1·1835	5·0247	5·1623	11·20
" 11 30 "	76 46·5								
to 11 0 a.m.	76 46·2	76 45·1	July Aug.	30 11	12 15 p.m.	1·1720	4·9780	5·1141	11·09
" 12 0 noon	76 44·0								
to 11 0 a.m.	76 24·4	76 22·7	Aug.	1 13	12 15 p.m.	1·2008	4·9926	5·1369	11·14
" 11 30 "	76 21·1								

of Vibration," by which I made all my observations on the submarine boat and on armour-plated ships. In this instrument the needle is 3·2 inches long and 0·17 inch in breadth.

The magnet is suspended on a silk thread and vibrates in a wooden box, on the side of which there are the divisions in degrees to mark the angles formed by the turning-needle; for each observation I counted 700 vibrations. I found that for such a voyage this instrument answers the purpose.

The exact drawing of this instrument you will find in my book on the "Submarine Boat," 1867, Plate VI. Drawings 6 and 8 give half the size of the instrument, and drawing 7 the full size of the needle.

I know that General Sabine and the Royal Society are much interested now in the Northern Magnetic Observations, so I hastened to send the results to you, as I know that they cannot be printed soon in Russian.

I remain, your attached friend,

JOHN BELAVENETZ.

Cronstadt, $\frac{4}{16}$ November, 1870.

March 9, 1871.

General Sir EDWARD SABINE, K.C.B., President, in the Chair.

The following communications were read :—

- I. "Results of Seven Years' Observations of the Dip and Horizontal Force at Stonyhurst College Observatory, from April 1863 to March 1870." By the Rev. S. J. PERRY. Communicated by the President. Received January 23, 1871.

(Abstract.)

The object of the present paper is to bring further evidence to bear upon an important question of terrestrial magnetism.

The existence of a sensible semiannual inequality in the earth's magnetic elements, dependent on the position of the sun in the ecliptic, was deduced by General Sir Edward Sabine from a discussion in 1863 of a continuous series of the monthly magnetic observations taken at Kew. A previous reduction of observations made at Hobarton and at Toronto had first suggested the idea, and a new confirmation of the results has lately been obtained by Dr. Balfour Stewart from subjecting a second series of Kew observations to the same tests as before. The observations, which form the basis of the present discussion, extend over the period from March 1863 to February 1870, during which time the same instruments have been in constant use. These are a Jones unifilar and a dip-circle by Barrow, both tested at Kew, and a Frodsham chronometer. Sir Edward Sabine, who made the Stonyhurst Observatory one of his magnetic stations

in the English survey in 1858, greatly encouraged the undertaking of monthly magnetic observations, and the Rev. A. Weld procured in consequence the instruments still in use. Only occasional observations were made with these instruments for some years, and it was only in 1863 that a continuous series of monthly determinations of the magnetic elements was started by the Rev. W. Sidgreaves. He observed regularly until September 1868, when I returned to my former post at the Observatory, and I have continued the same work ever since.

A stone pillar was at first erected for the magnetic instruments in the open garden, and this remained in use from 1858 until the beginning of 1868, when a most convenient hut of glass and wood was built for the instruments in a retired corner of the College garden. This alteration was rendered necessary from the placing of iron rails in the vicinity of the old pillar; and although it introduces into the results a correction for change of station, it has the great advantage of securing immunity from disturbance for the future.

Considering the object in view in drawing up this reduced form of the dip and horizontal-force observations, I have judged it advisable to adhere strictly to the tabular forms in which the matter has been presented in previous discussions of a similar nature. Each element is the subject matter of these tables. In the first are the monthly values of the element, the deduced mean value, and its secular variation. Next in order comes the calculation of the semiannual inequality. The residual errors, and consequent probable weights of the observations and results, compose the third and last Table.

The yearly mean values of the horizontal force are found to vary progressively from 3.5926 to 3.6178 in British units, the mean for Oct. 1st, 1866, being 3.6034, with a secular acceleration of 0.0042. Calculating from the monthly Tables the mean value of the horizontal force for the six months from April to September, and for the semiannual period from October to March, we find the former to be 0.0005 in excess over the latter, showing that this component of the intensity is greater during the summer than during the winter months. Treating the dip observations in a precisely similar way, we obtain $69^{\circ} 45' 21''$ as the mean value of this element for October 1st, 1866, subject to a secular diminution of $1' 49'' \cdot 2$; the extreme yearly means being $69^{\circ} 48' 47''$ and $69^{\circ} 37' 52''$. The resulting excess of $10''$ for the winter months in the computed semiannual means is so small, that the observations tend mainly to show that the effect of the sun's position is not clearly manifested by any decided variation in the dip. Deducing the intensity from the above elements, we obtain for the summer months the value 10.4136, whilst that for the winter months is 10.4128. The intensity of the earth's magnetic force would thus appear to increase with the sun's distance, but the difference is not large enough to have more than a negative weight in the question under discussion. This weight, moreover, is lessened by the slight uncer-

tainty arising from the probable disturbing causes at the first magnetic station.

It is hoped that a second series of observations at the new station will throw greater light on the fact of the sun's influence on terrestrial magnetism, by either confirming the results obtained above, or by adding fresh weight to the conclusions arrived at by the President of the Royal Society.

II. "Preliminary Notice on the Production of the Olefines from Paraffin by Distillation under Pressure." By T. E. THORPE, Ph.D., Professor of Chemistry in Anderson's University, Glasgow, and JOHN YOUNG. Communicated by Professor ROSCOE, F.R.S. Received February 2, 1871.

When paraffin is exposed to a high temperature in a closed vessel, it is almost completely resolved, with the evolution of but little gas, into hydrocarbons which remain liquid at the ordinary temperature.

This reaction will undoubtedly afford the most important insight into the constitution of this body.

Accordingly we have repeated this conversion on a large scale, and from about $3\frac{1}{2}$ kilograms of paraffin melting at $44^{\circ}5$ C. (prepared from shale) we have obtained nearly four litres of liquid hydrocarbons. This mixture of hydrocarbons commences to boil at about 18° C., but the quantity coming over below 100° C. is comparatively small; by far the greater portion boils between 200° and 300° . A preliminary separation shows that the four litres are made up of hydrocarbons boiling

	litres.
Between 200° and 300°	2.7
„ 100° and 200°	1.0
Below 100°	0.3
	—
	4.0

Up to the present we have principally occupied ourselves with the investigation of the fraction boiling below 100° , and have obtained conclusive evidence that it is mainly composed of *olefines*, the proportion of members of the $C_n H_{2n} + 2$ series being but small. By repeated fractionations over sodium we obtained perfectly colourless liquids boiling about 35° and 65° , which were attacked by bromine in the cold with the greatest energy. On adding the bromine slowly and in minute drops, and carefully cooling the hydrocarbon by a mixture of snow and salt, scarcely a trace of hydrobromic acid was produced. The portion boiling at 36° may be either amylhydride or amylene, or a mixture of both; the avidity with which the bromine combines with it shows that the latter body must be present in considerable quantity. As soon as the drops of bromine permanently

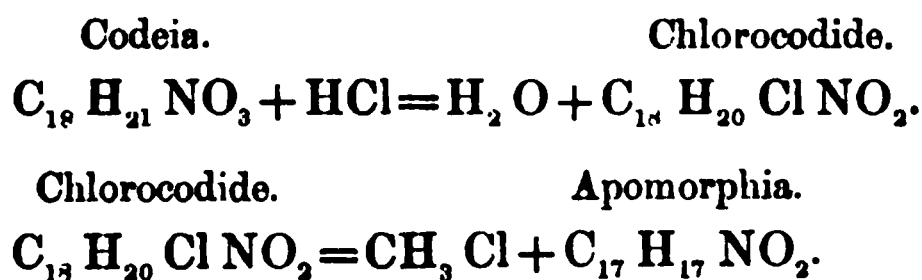
coloured the liquid, it was submitted to distillation. Only a small portion came over below 40° ; the thermometer rose rapidly to 180° , and nearly the whole of the bromine-compound distilled at 184° to 188° . This substance is amylene bromide, $C_6 H_{10} Br_2$; Wurtz gives the boiling-point of this body at about 180° . The portion therefore boiling at 35° is mainly amylene.

Exactly similar results were obtained from the portion boiling at 65° to 70° . This, from its boiling-point, may be either $C_8 H_{12}$ or $C_8 H_{14}$, or a mixture of both. Bromine disappears instantly on adding it to the carefully cooled liquid, and on distillation by far the greater portion is found to have combined with the halogen. The bromide thus obtained distils with slight decomposition about 195° . Pelouze and Cahours found that hexylene bromide, $C_6 H_{12} Br_2$, boiled at 192° to 198° .

We are at present engaged in the further investigation of this subject, and hope shortly to lay our results before the Royal Society.

III. "Contributions to the History of the Opium Alkaloids. Part I.—On the Action of Hydrobromic Acid on Codeia." By C. R. A. WRIGHT, D.Sc. Communicated by Professor ROSCOE F.R.S. Received February 6, 1871.

It has been shown by the late Dr. A. Matthiessen, in conjunction with the writer *, that when codeia is heated with a large excess of strong hydrochloric acid the following reactions successively take place:—



It appeared of interest to examine the action of hydrobromic acid under similar circumstances, and for this purpose Messrs. Macfarlane, of Edinburgh, with their wonted liberality, put a considerable quantity of pure codeia at the writer's disposal.

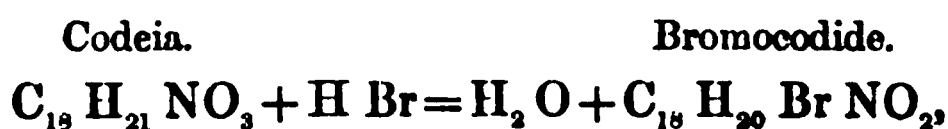
The aqueous hydrobromic acid employed was obtained by the action of $H_2 S$ on Br in presence of water, and subsequent rectification over pulverized $K Br$; it was free from $SO_4 H_2$ and other sulphur compounds, had a sp. gr. of about 1.5, and contained about 48 per cent. of $H Br$.

When codeia is heated with from three to six times its weight of this acid, either on a water-bath or to gentle ebullition over a flame, the liquid, which at first produces no precipitate with solution of carbonate of soda, gradually darkens in colour, and acquires the property of yielding a dense white precipitate with this reagent. No appreciable quantity of methyl

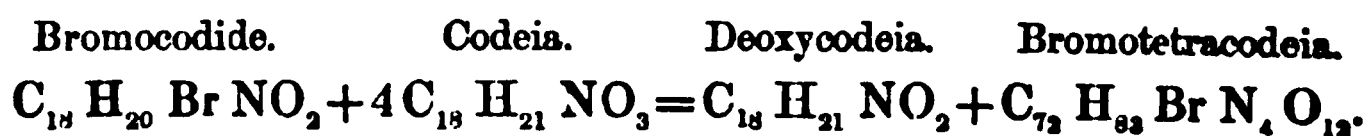
* Proc. Roy. Soc. vols. xvii. p. 460, xviii. p. 83.

bromide is evolved during the first stages of this change, but subsequently this body is produced in some little quantity.

The precipitate thrown down by carbonate of soda before this further change ensues, appears to consist of a variable mixture of at least three substances, two of which are readily soluble in ether, while the third is but sparingly soluble in that menstruum; all are bases, the one insoluble in ether, and one of those soluble containing bromine: the one apparently first formed is produced by a reaction precisely analogous to that whereby chlorocodide is generated, viz.—



and is therefore termed bromocodide; this base appears to be acted on further with great ease, giving rise ultimately to the other two, the first of which has the constitution of codeia less one equivalent of oxygen, or $C_{19}H_{21}NO_2$, and is therefore provisionally named Deoxycodide; whilst the second has the composition of four molecules of codeia coalesced together, one of the 84 H atoms in the product being replaced by Br; it is therefore provisionally termed Bromotetracodide, the simplest mode of representing the simultaneous formation of these two bases being as follows:—



Owing to the ease with which bromocodide is altered, it is a matter of some difficulty to obtain it in even an approximately pure condition, as the complete separation of deoxycodide appears impracticable when this base has once been produced. The product of the action of three parts 48 per cent. acid on one part codeia on the water-bath for from one to two hours is precipitated by excess of sodium carbonate and the precipitate collected on filters; unaltered codeia is for the most part separated thus, being contained in the filtrate. Extraction of the mass with ether and agitation of the ethereal solution with HBr furnishes crude bromocodide hydrobromate, which may be purified by a repetition of the process, fractional precipitation being resorted to to get rid of traces of colouring-matters: the purified hydrobromate thus obtained was a viscid colourless liquid which utterly refused to crystallize, and dried up to a gum-like mass over SO_4H_2 . Dried at 100° , the powdered gum gave these numbers*:

0.3500 grm. gave 0.6340 CO_2 and 0.1580 H_2O .

0.230 grm, boiled with NO_3H and $AgNO_3$, gave 0.1900 $AgBr$.

* All combustions given in this paper were made with lead chromate and finished in a stream of dry oxygen.

Calculated.			Found.	
C ₁₈	216	48·76	49·40	
H ₂₁	21	4·74	5·01	
Br ₂	160	36·12	35·08
N	14	3·16		
O ₂	32	7·22		
<hr/>			<hr/>	
C ₁₈ H ₂₀ Br NO ₂ HBr	443	100·00		

The slight excess of carbon and deficiency in bromine thus found are doubtless due to the presence of a little deoxycodeia, the hydrobromate of which requires 59·34 per cent. carbon and 21·98 per cent. Br. Another specimen of bromocodide hydrobromate, prepared as above from the product of three hours' digestion at 100° of one part codeia and three parts 48 per cent. HBr, yielded numbers indicating 51·6 per cent. carbon, 5·3 and 33·4 per cent. Br; whilst a repetition of the purification process scarcely altered the numbers. Owing to the great difficulty in preparing the pure salt in quantity, no attempt to isolate and analyze the base itself was made, the more so that the precipitate thrown down by carbonate of soda from the pure hydrobromate appeared to tally in every respect with the chlorocodide formerly examined; their qualitative reactions, too, are identical.

The crude bromocodide hydrobromate obtained after five or six hours' digestion of codeia with from three to five times its weight of 48 per cent. HBr deposited, on standing for some days, crystals not readily soluble in cold water; recrystallized several times from boiling water, minute snow-white crystals were ultimately obtained; these slightly darkened on drying over SO₄ H₂, and more so at 100° and gave the following numbers on analysis :—

- 0·3565 grm. gave 0·7760 CO₂ and 0·1960 H₂ O.
- 0·3245 grm. gave 0·7045 CO₂ and 0·1790 H₂ O.
- 0·2200 grm. burnt with soda-lime gave 0·0570 Pt.
- 0·1380 grm. boiled with NO₃ H and Ag NO₃ gave 0·0700 AgBr.

These numbers agree with those calculated for deoxycodeia hydrobromate, as the following comparison shows :—

Calculated.			Found.		
C ₁₈	216	59·34	59·36	59·21	
H ₂₂	22	6·05	6·11	6·13	
N	14	3·84	3·69
O	32	8·79			
Br	80	21·98 21·59
<hr/>			<hr/>		
C ₁₈ H ₂₁ NO ₂ HBr	364	100·00			

The yield of this base from the codeia used being but small (about 4 per cent.), no attempt was made to isolate the base itself; carbonate of soda throws down from the hydrobromate solution a white precipitate which is soluble in alcohol, ether, benzol, and chloroform; by exposure to air it rapidly becomes coloured, and finally acquires a very dark green tint. Its qualitative reactions are identical with those of apomorphia; the colour-reactions of the two with Fe_2Cl_6 , NO_3H , and $\text{SO}_4\text{H}_2 + \text{K}_2\text{Cr}_2\text{O}_7$, being indistinguishable when examined side by side. Its physiological effects, however, are different; three-tenths of a grain of the hydrobromate administered by the mouth to a dog producing no appreciable effect, whilst a much less dose of apomorphia produces speedy vomiting.

The third base is conveniently obtained, as hydrobromate, by treating codeia with three times its weight of 48 per cent. HBr for two hours on the water-bath, precipitating the product (diluted with water) by excess of carbonate of soda, collecting on filters, and well draining from the mother-liquors, and finally extracting with ether until scarcely anything more is taken up; care must be taken to have as little watery fluid as possible present, otherwise the insoluble substance forms a sort of lather, on agitation from which the ether will not separate. The insoluble substance is then dissolved in the least possible quantity of weak hydrobromic acid and fractionally precipitated by cautious addition of stronger acid; the second precipitate is dissolved up in water, in which it is readily soluble, and a few drops of carbonate-of-soda solution added. The filtrate from this yields, with strong HBr , nearly white flakes, which are wholly void of crystalline character under the microscope. These remain solid at 100° if previously completely dried over SO_4H_2 ; but if warmed while moist, become a more or less coloured tar. Dried at 100° , the following numbers were obtained:—

0.3440 grm. gave 0.6810 CO_2 and 0.1740 H_2O .

0.3425 grm. gave 0.6685 CO and 0.1680 H_2O .

0.5615 grm. burnt with soda-lime gave 0.1310 Pt.

0.3200 grm. boiled with NO_3H and AgNO_3 gave 0.1330 AgBr .
and 0.0315 Ag .

Calculated.			Found.		
C_{72}	864	54.03	53.99	53.23	
H_{87}	87	5.44	5.61	5.45	
N_4	56	3.50	3.33
O_{12}	192	12.01			
Br_5	400	25.02 24.97
<hr/>			<hr/>		
$\text{C}_{72}\text{H}_{83}\text{BrN}_4\text{O}_{12}, 4\text{HBr}$	1599	100.00			

Carbonate of soda throws down from the hydrobromate a nearly white

precipitate, which rapidly darkens, and finally turns a deep green, nearly black. Dried at 100° rapidly, the product gave the following numbers, which fall below those required for the formula $C_{72}H_{83}BrN_4O_{12}$, but which agree with those required for a similar formula but containing more oxygen :—

0·3810 gram. gave 0·8460 CO_2 and 0·2080 H_2O .

0·4430 gram. boiled with $AgNO_3$ and NO_3H gave 0·059 $AgBr$.

Calculated			Found.	
C_{72}	864	60·89	60·56	
H_{83}	83	5·85	6·07	
Br	80	5·64	5·67
N_4	56	3·95		
O_{21}	336	23·67		
<hr/>			<hr/>	
$C_{72}H_{83}BrN_4O_{12} + O_9$	1419	100·00		

It hence appears that the free base rapidly absorbs oxygen. In confirmation of this, 0·11 gram. of the hydrobromate treated with caustic potash and injected by a pipette into 15 cubic centims. of air over mercury absorbed 0·9 cubic centim. in the course of an hour, or 6 per cent. of the total volume of the air; the salts, however, when dry, may be kept without alteration, and only slowly darken by exposure to air when moist.

This welding together of four molecules is not wholly without parallel in the history of the opium alkaloids; and their derivatives thus opianic acid heated * furnishes a body containing four times as much carbon as the original acid; thus



The qualitative reactions of bromotetracodeia appear to be identical with those of bromo- and chlorocodide. The base itself, when freshly precipitated, is slightly soluble in water, being thrown down again by addition of strong brine; in ether and benzol it is almost insoluble, and in alcohol but sparingly soluble.

When crude bromotetracodeia, got by extraction with ether of the mixture of bases thrown down by carbonate of soda, is dissolved in weak hydrochloric acid, and precipitated twice or thrice by excess of stronger acid, nearly white flakes are ultimately obtained, resembling in all their physical properties the bromohydrobromate of tetracodeia. These flakes, however, contain no bromine, the absence of this element being ascertained by the negative results obtained on examining with chlorine-water and ether the acidified solutions of the lime-salts got by combustion with quicklime, and of the sodium-salts got by boiling with NO_3H and $AgNO_3$, and fusing with carbonate of soda the silver-salts thus got. Dried over SO_4H_2 and finally at 100°, this body gave numbers indicating a base of constitution

* Matthiessen and Wright, Proc. Roy. Soc. vol. xvii. p. 341.

analogous to that of bromotetracodeia ; it may therefore be termed chlorotetracodeia.

Specimen A.—0.3880 grm. gave 0.1970 AgCl.

0.3645 grm. gave 0.8395 CO₂ and 0.2120 H₂O.

0.3940 grm. burnt with soda-lime gave 0.1080 Pt.

Specimen B.—0.4460 grm. gave 1.0150 CO₂ and 0.2560 H₂O.

0.2350 grm. gave 0.1250 AgCl.

Calculated.				Found			
				Specimen A.		Specimen B.	
C ₇₂	864	62.77		62.81	62.07	
H ₈₇	87	6.32		6.46		6.38	
N ₄	56	4.07		3.90		
O ₁₂	192	13.94					
Cl ₃	177.5	12.90		12.56	13.16
<hr/>							
C ₇₂ H ₈₃ ClN ₄ O ₁₂ 4HCl	1376.5	100.00					

Specimen A had been three times precipitated by HCl in large excess, while specimen B had only been thrown down twice, and probably retained a trace of bromotetracodeia.

Specimen A converted into platinum-salt gave the following numbers after drying at 100°.

0.4215 grm. gave 0.0810 Pt = 19.22 per cent.

The formula C₇₂ H₈₃ ClN₄ O₁₂ 4 HCl, 2 PtCl₄ requires 19.18 per cent.

Like bromotetracodeia, the free base appears to absorb oxygen with avidity. Dried as rapidly as possible at 100°, the precipitate thrown down by carbonate of soda gave these numbers :—

0.3880 grm. gave 0.9190 CO₂ and 0.2230 H₂O.

0.3100 „ 0.0330 AgCl.

Calculated.				Found.	
C ₇₂	864	64.74		64.59	
H ₈₃	83	6.22		6.38	
N ₄	56	4.20			
O _{12½}	296	22.18			
Cl	35.5	2.66		2.64
<hr/>					
C ₇₂ H ₈₃ ClN ₄ O ₁₂ + O _{6½}	1334.5	100.00			

In all its physical and chemical properties chlorotetracodeia closely resembles bromotetracodeia : their qualitative reactions are identical ; they have an intense bitter taste and apparently but slight physiological action, at any rate in small doses.

My thanks are due to Mr. J. L. Bell, in whose laboratory the above experiments were carried out.

March 16, 1871.

General Sir EDWARD SABINE, K.C.B., President, in the Chair.

The following communications were read :—

- I. "Description of *Ceratodus*, a genus of Ganoid Fishes, recently discovered in rivers of Queensland, Australia." By ALBERT GÜNTHER, M.A., Ph.D., M.D., F.R.S. Received February 7, 1871.

(Abstract.)

After some introductory remarks the author proceeds to give a description of the external characters which appear to indicate the existence of two species, viz. *Ceratodus forsteri*, with fewer and larger, and *Ceratodus miolepis* with smaller and more numerous scales. The microscopical structure of the scales and teeth is treated of in two separate chapters, the latter being compared with the fossils from the Triassic and Jurassic formations, and found to be identical. The resemblance to the dentition of *Protopterus*, *Psammodus*, *Dipterus* and other fossil genera is pointed out.

The *skeleton* resembles in its general characters, as well as in numerous points of detail, so much that of *Lepidosiren*, that from this part alone the conclusion must be drawn that these genera belong to the same natural group of fishes. It is notochordal, all its parts having a cartilaginous basis, more or less incompletely covered by thin osseous lamellæ.

The ossifications of the skull are but few in number, and may be designated thus :—*ethmoid*; a pair of *frontals* separated by a single "*sclero-parietal*"; basal, with a tooth-bearing *pterygo-palatine* on each side, the latter bones being suturally united in front; *vomer* cartilaginous, tooth-bearing. *Maxillary* and *intermaxillary* elements are not developed, replaced by facial cartilages which are confluent with the suborbital ring, all these parts being cavernous. *Tympanic pedicle* cartilaginous, with ossified lamella (*os quadratum*) and double condyle. *Mandible* with an *articular* and *dentary* lamella. *Præoperculum* a rudimentary moveable cartilage. A well-developed *operculum* and styliform *suboperculum*. *Hyoid arch* more complex than in *Lepidosiren*, consisting of a pair of ceratohyals, a basi- and glosso-hyal. *Branchial apparatus* composed of five arches, of which the last is rudimentary; not differing from the Teleosteous type, but cartilaginous. In a vertical section of the head the parts of the brain-cavity and of the *acoustic* cavity (which is entirely enclosed in the skull) are explained. A pituitary gland is present.

The notochord forms the base for about 68 sets of *apophyses*, 27 of which bear ribs. The various modifications in the different parts of the column are described in detail; and more especially attention is directed to the first rib, which is very similar to that of *Lepidosiren*, where, from its more intimate connexion with the skull, it was interpreted in various

ways, for instance by Mr. Parker as the "large first pharyngo-branchial." Arrangement and detachment of dermoneurals as in *Lepidosiren*.

The scapular arch and pelvis are more developed than, but typically entirely identical with, those of *Lepidosiren*.

The *paddles* are supported by a cartilaginous axial skeleton, that is, by a longitudinal series of joints, with lateral divergent articulated branches, each joint having two of these branches. The relations of this singular structure to the corresponding parts in *Lepidosiren* and Selachians are explained; and there is no doubt that the Ganoids of the Devonian epoch, with *acutely* lobate fins, had their paddles supported by a similar internal skeleton.

Eye without falciform process or choroid gland.

Heart.—The arrangements of the interior of the ventricle and single atrium, and the external appearance of the bulbus arteriosus, are very similar to the same parts in *Lepidosiren*; but the valvular arrangement of the bulbus is more "Ganoid," though considerably modified. We find at a short distance from the origin of the bulbus, first, a single, cartilaginous, papillary valve worked by a special muscle, then a transverse series of four small short valves (sometimes reduced to papillæ), then a transverse series of four oblong raised strips (rudimentary valves), finally a transverse series of four well-developed "Ganoid" valves. Four *arcus aortæ* enter the four gills, without sending off branches, and four *venæ branchiales* are collected into the *aorta descendens*.

A description of the principal portions of the circulatory system follows.

The *gills* are completely developed, four in number, lamellated. The pseudobranchia does not receive its blood from the heart; thus an "opercular gill" is absent as well as spiracles.

The *lung* is single, but its cavity divided into two symmetrical halves, each with about thirty cellular compartments; pneumatic duct and glottis as in *Lepidosiren*; its dorsal artery is a branch of the *A. cæliaca*; and its vein enters the atrium separately from the sinus venosus.

The most important points of the structure of the remaining soft organs are the following:—the *intestinal tract* is perfectly straight and very wide, with a complete spiral valve, along the axis of which large glands are imbedded; the stomach is indicated only by a shallow double pyloric fold; there are no pyloric appendages, but a glandular mass appears to represent the spleen. Not only the *liver*, but also the paired, lobed *kidneys* are provided with a portal system. The two ureters enter by a single opening a small urinal cloaca situated at, and partly confluent with, the back of the rectum. *Testicles* without developed vas deferens, which appears to be represented by a blind duct, traversing the interior of the testicle, and receiving the semen from the canaliculi seminiferi. *Ovaries* transversely laminate, the laminæ being the bearers of the stroma in which very small ova are developed; the ova fall into the abdominal cavity, and are expelled by a pair of wide peritoneal slits behind the vent. But there

are also a pair of narrow oviducts, with or without a narrow peritoneal opening, each confluent with the ureter of its side.

In the concluding chapters it is shown :—

1. That *Ceratodus* and *Lepidosiren* (*Protopterus*) are more nearly allied to each other than to any third living fish, that they are well-marked modifications of the same (Dipnoous) type, the latter genus diverging more towards the Amphibians than the former.

2. That the difference in the arrangement of the valves of the bulbus arteriosus cannot longer be considered of sufficient importance to distinguish the *Dipnoi* as a subclass from the *Ganoidei*; but that the *Dipnoi* may be retained as a suborder of *Ganoidei*.

3. That the suborder *Dipnoi* may be characterized as Ganoids with the nostrils within the mouth, with paddles supported by an axial skeleton, with lungs and gills and notochordal skeleton, and without branchiostegals.

4. That a comparison of *Teleostei*, *Chondropterygii*, and *Ganoidei* shows that the two latter divisions, hitherto regarded as subclasses, are much more nearly allied to each other than to the *Teleostei*, which were developed in much more recent epochs; and therefore that they should be united into one subclass—*Palæichthyes*—characterized thus: heart with a contractile bulbus arteriosus; intestine with a spiral valve; optic nerves non-decussating.

5. That there is very strong evidence that the suborder *Dipnoi* was represented in the Devonian and Carboniferous epochs by the genus *Dipterus* (?= *Ctenodus*); but that, although *Dipterus* has internal nostrils and even a pair of vomerine teeth (beside the molars) like the living *Dipnoi*, it must be placed as the type of a separate family of this suborder, on account of its heterocercy.

6. That the evidence with regard of *Phaneropleuron* (Huxley) is less conclusive; and that *Tristichopterus* (Egerton), with the complete segmentation of its vertebral column, must be excluded from this suborder.

7. That the suborder *Crossopterygii* (Huxley) contains two distinct types of "lobate fin," viz. the "obtusely lobate," with a transverse series of carpal cartilages, and the "acutely lobate" with an axial skeleton. Only the latter type agrees with the structure of the Dipnoous limb. But *Polypterus*, *Cœlacanthus*, &c., which are provided with fins of the former type, are genera sufficiently distinguished also by other characters, to be placed into a separate suborder.

- II. "On the formation of some of the Subaxial Arches in Man."
By GEORGE W. CALLENDER, Assistant Surgeon to, and Lecturer on Anatomy at St. Bartholomew's Hospital. Communicated by J. PAGET, F.R.S. Received February 17, 1871.

(Abstract.)

In the term subaxial arches is included all those which grow out in front of the notochord. The first forms the nasal passages, the second forms chiefly the superior maxilla, the third is the mandibular, the fourth the lingual, the fifth the hyoid, the sixth the laryngeal, whilst the seventh, which is distinguished as the exoccipital arch, forms the shoulder-girdle and the thoracic extremity of either side.

The consideration of the connexion of the first four with the cranial cartilages is for the present deferred; and this communication relates to those arches which grow into the cervical region, and to that period of their growth which lies between the fifth and twelfth weeks of foetal life.

The fourth subaxial arch, the lingual, grows out below the mandibular, and bears upon its anterior extremity the tissue which develops into the tongue. Its connexion with this anterior portion ceases to be recognized at an early period, and about the eleventh week it consists of five portions, (1) cartilage from the base of the skull, (2) a short piece of membrane, (3) a second very small rod of cartilage, these forming about one-half of its length, (4) a long strip of membrane, and (5) a nodule of cartilage within the lesser or anterior horn of the hyoid bone.

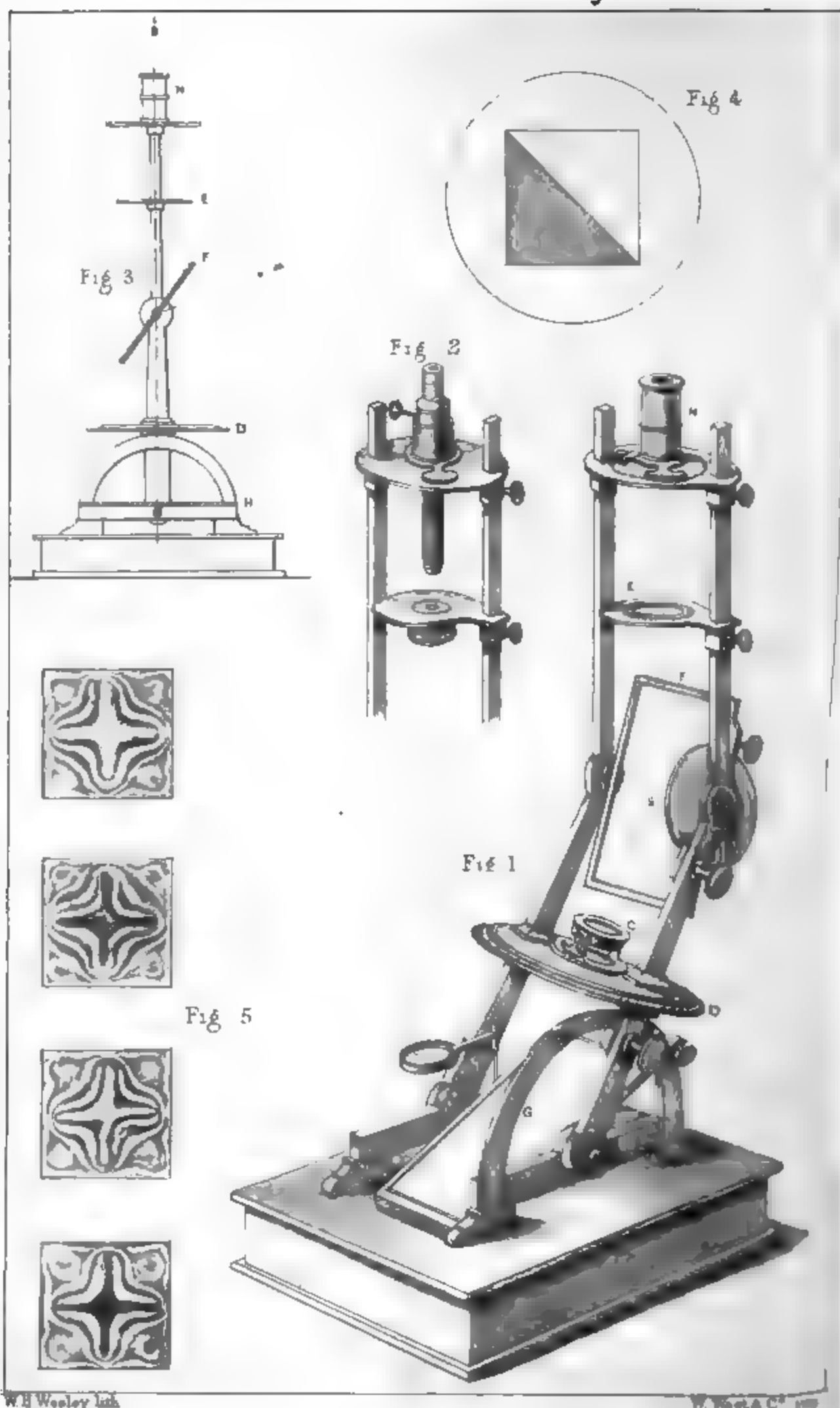
The fifth subaxial arch, the hyoid, grows in common with the sixth as a layer of membrane from the basioccipital region. The posterior portion of it forms the middle constrictor muscle; the remainder is cartilaginous, and grows into the greater or posterior horn and the body of the hyoid bone.

The sixth subaxial arch, the laryngeal, begins in membrane which forms the inferior constrictor. Rising up and thickening in the front of the neck, it encloses the pharynx; and its inner layer develops a septum, which separates this tube from the larynx. In front, and between this inner layer and that in which the constrictor is formed, a mass of thick granular tissue becomes cartilage, and here the chief cartilages of the larynx are formed. The details of their formation are referred to.

The thyroid body is developed in this arch, and it serves as a girdle to surround and keep in place the continuation of the air-tube towards the thorax. Its relation to the branchial arches is also referred to.

After mentioning the reason for calling the seventh arch the exoccipital, its growth from the basioccipital and exoccipital cartilage regions is described, with its ending in two processes which grow out as the clavicle and the scapula. The relations of the clavicle to the sternum and first rib are related, as also its change in direction from a nearly vertical to a hori-





zontal position. The curling round of the scapula-rod is described, and the outgrowth from the rod of plates of bone bounded by the acromial, glenoid, and coracoid borders. The relations of the sterno-mastoid, trapezius, and levator anguli scapulæ muscles are referred to. The growth of the glenoid cavity outwards from the acromion and coracoid is noticed at about the eleventh week, at which period the scapula has acquired its chief permanent characters.

March 23, 1871.

General Sir EDWARD SABINE, K.C.B., President, in the Chair.

The following communications were read :—

- I. "Experiments on the Successive Polarization of Light, with the description of a new Polarizing Apparatus." By Sir CHARLES WHEATSTONE, F.R.S. Received Feb. 2, 1871.

I.

The term successive polarization was applied by Biot to denote the effects produced when a ray of polarized light is transmitted through a plate of rock-crystal cut perpendicularly to the axis, or through limited depths of certain liquids. In these cases the plane of polarization is found to be changed on emergence, and differently for each homogeneous ray ; so that, when white light is employed, on turning the analyzer round continuously in one direction different colours successively appear, rising or falling in the scale according to the nature of the substance.

If, while the analyzer is turned from left to right, the tints ascend (*i. e.* follow the order R, O, Y, G, B, P, V), the substance is said to exhibit right-handed successive polarization, but if the tints descend, the successive polarization is said to be left-handed.

These phenomena were satisfactorily explained by Fresnel in the following way. The incident polarized ray, instead of resolving itself into two plane-polarized rays at right angles to each other, as in the ordinary cases of dipolarization, resolves itself in these instances into two circularly polarized rays, one right-handed the other left-handed, which are transmitted with different velocities ; each homogeneous ray, thus resolved into two opposite circularly polarized pencils, on emergence composes a ray polarized in a single plane, the deviation of which from the primitive plane of polarization depends on the difference of phase of the two circularly polarized rays on emergence.

The rotation of the planes of polarization is from left to right, or from right to left, according to whether the right-handed or left-handed circular rays are transmitted with the greater velocity.

II.

The term dipolarization, proposed by Dr. Whewell to express the

bifurcation which a ray of polarized light suffers when it is transmitted through a crystallized plate, is a very appropriate one; but as there are different kinds of such separation, we may designate plane dipolarization the resolution into two plane-polarized rays at right angles to each other, and circular dipolarization the resolution into two circularly polarized rays, one right-handed, the other left-handed. In like manner the term elliptic dipolarization may be employed to represent the phenomena shown by transmitting a polarized ray through a plate of rock-crystal obliquely to the axis.

The object of the present communication is to make known another means of producing successive polarization, both right-handed and left-handed, which, equally with the well-known modes, may be proved to arise from the interference of two opposite systems of circularly polarized rays.

III.

The polarizing-apparatus which I have employed for the experiments I am about to detail is represented by Pl. IV.

A plate of black glass, G, is fixed at an angle of 3° to the horizon. The film to be examined is to be placed on a diaphragm, D, so that the light reflected at the polarizing-angle from the glass plate shall pass through it at right angles, and, after reflection at an angle of 18° from the surface of a polished silver plate S, shall proceed vertically upwards. N is a Nicol's prism, or any other analyzer, placed in the path of the second reflection. The diaphragm is furnished with a ring, moveable in its own plane, by which the crystallized plate to be examined may be placed in any azimuth. C is a small moveable stand, by means of which the film to be examined may be placed in any azimuth and at any inclination; for the usual experiments this is removed.

If a lamina of quartz cut parallel to the axis, and sufficiently thin to show the colours of polarized light, be placed upon the diaphragm so that its principal section (*i. e.* the section containing the axis) shall be 45° to the *left* of the plane of reflection, on turning the analyzer from left to right, instead of the alternation of two complementary colours at each quadrant, which appear in the ordinary polarizing apparatus, the phenomena of successive polarization, exactly similar to those exhibited in the ordinary apparatus by a plate of quartz cut perpendicular to the axis, will be exhibited; the colours follow in the order R, O, Y, G, B, P, V, or, in other words, ascend as in the case of a right-handed plate of quartz cut perpendicularly to the axis. If the lamina be now either inverted, or turned in its own plane 90° , so that the principal section shall be 45° to the right of the plane of reflection, the succession of the colours will be reversed, while the analyzer moves in the same direction as before, presenting the same phenomena as a left-handed plate of quartz cut perpendicularly to the axis. Quartz is a positive doubly refracting crystal; and in it consequently the ordinary index of refraction is smaller than the

extraordinary index. But if we take a lamina of a negative crystal, in which the extraordinary index is the least, as a film of Iceland spar split parallel to one of its natural cleavages, the phenomena are the reverse of those exhibited by quartz: when the principal section is on the *left* of the plane of reflection the colours descend, and when it is on the *right* of the same plane the colours ascend, the analyzer being turned from left to right.

It has been determined that the ordinary ray, both in positive and negative crystals, is polarized in the principal section, while the extraordinary ray is polarized in the section perpendicular thereto. It is also established that the index of refraction is inversely as the velocity of transmission. It follows from the above experimental results, therefore, that when the resolved ray whose plane of polarization is to the left of the plane of reflection is the quickest the successive polarization is right-handed, and when it is the slowest the successive polarization is left-handed—in the order R, O, Y, G, B, P, V, and in the second case in the reversed order.

The rule thus determined is equally applicable to laminæ of biaxal crystals.

As selenite (sulphate of lime) is an easily procurable crystal, and readily cleavable into thin laminæ capable of showing the colours of polarized light, it is most frequently employed in experiments on chromatic polarization. The laminæ into which this substance most readily splits, contain in their planes the two optic axes; polarized light transmitted through such laminæ is resolved in two rectangular directions, which respectively bisect the angles formed by the two optic axes: the line which bisects the smallest angle is called the intermediate section; and the line perpendicular thereto which bisects the supplementary angle is called the supplementary section. These definitions being premised, if a film of selenite is placed on the diaphragm with its intermediate section to the left of the plane of reflection, the successive polarization is direct or right-handed; if, on the contrary, it is placed to the right of that plane, the successive polarization is left-handed. The ray polarized in the intermediate section is therefore the most retarded; and as that section is considered to be equivalent to a single optic axis, the crystal is positive.

In one kind of mica the optic axes are in a plane perpendicular to the laminæ. They are inclined $22\frac{1}{2}^{\circ}$ on each side the perpendicular within the crystal, but, owing to the refraction, are seen respectively at an angle of $35^{\circ}.3$ therefrom. The principal section is that which contains the two optic axes. If the film is placed on the diaphragm with its principal section inclined 45° to the left of the plane of reflection, the successive polarization is right-handed. The ray, therefore, polarized in the section which contains the optic axes is the one transmitted with the greatest velocity.

Films of uniaxal crystals, whether positive or negative, and of biaxal

crystals, all agree therefore in this respect :—that if the plane of polarization of the quickest ray is to the left of the plane of reflection, the successive polarization is right-handed when the analyzer moves from left to right ; and if it is to the right of the plane of reflection, other circumstances remaining the same, the successive polarization is left-handed.

It must be taken into consideration that the principal section of the film is inverted in the reflected image ; so that if the plane of polarization of the quickest ray in the film is to the left of the plane of reflection, it is to the right of that plane in the reflected image.

IV.

It may not be uninteresting to state a few obvious consequences of this successive polarization in doubly refracting laminæ, right-handed and left-handed according to the position of the plane of polarization of the quickest ray. They are very striking as experimental results, and will serve to impress the facts more vividly on the memory.

1. A film of uniform thickness being placed on the diaphragm with its principal section 45° on either side the plane of reflection, when the analyzer is at 0° or 90° the colour of the film remains unchanged, whether the film be turned in its own plane 90° , or be turned over so that the back shall become the front surface ; but if the analyzer be fixed at 45° , 135° , 225° , or 315° , complementary colours will appear when the film is inverted from back to front, or rotated in its own plane either way 90° .

2. If a uniform film be cut across and the divided portions be again placed together, after inverting one of them, a compound film (fig. 4) is formed, which, when placed on the diaphragm, exhibits simultaneously both right-handed and left-handed successive polarization. When the analyzer is at 0° or 90° the colour of the entire film is uniform ; as it is turned round the tints of one portion ascend, while those of the other descend ; and when the analyzer is at 45° or $\approx 90^\circ + 45^\circ$, they exhibit complementary colours.

3. A film increasing in thickness from one edge to the other is well suited to exhibit at one glance the phenomena due to films of various thicknesses. It is well known that such a film placed between a polarizer and an analyzer will show, when the two planes are parallel or perpendicular to each other, and the principal section of the film is intermediate to these two planes, a series of parallel coloured bands, the order of the colours in each band from the thick towards the thin edge being that of their refrangibilities, or R, O, Y, G, B, P, V. The bands seen when the planes are perpendicular are intermediate in position to those seen when the planes are parallel ; on turning round the analyzer these two systems of bands alternately appear at each quadrant, while in the intermediate positions they entirely disappear.

Now let us attend to the appearances of these bands when the wedge-form film is placed on the diaphragm of the instrument, fig. 1. As the

analyzer is moved round, the bands advance toward or recede from the thin edge of the wedge without any changes occurring in the colours or intensity of the light, the same tint occupying the same place at every half revolution of the analyzer. If the bands advance toward the thin edge of the wedge, the successive polarization of each point is left-handed; and if they recede from it the succession of colours is right-handed; every circumstance, therefore, that with respect to a uniform film changes right-handed into left-handed successive polarization, in a wedge of the same substance transforms receding into advancing bands, and *vice versa*. These phenomena are also beautifully shown by concave or convex films of selenite or rock-crystal, which exhibit concentric rings contracting or expanding in accordance with the conditions previously explained.

4. Few experiments in physical optics are so beautiful and striking as the elegant pictures formed by cementing laminae of selenite of different thicknesses (varying from $\frac{1}{2000}$ to $\frac{1}{50}$ of an inch) between two plates of glass. Invisible under ordinary circumstances, they exhibit, when examined in the usual polarizing-apparatus, the most brilliant colours, which are complementary to each other in the two rectangular positions of the analyzer. Regarded in the instrument, fig. 1, the appearances are still more beautiful; for, instead of a single transition, each colour in the picture is successively replaced by every other colour. In preparing such pictures it is necessary to pay attention to the direction of the principal section of each lamina when different pieces of the same thickness are to be combined together to form a surface having the same uniform tint; otherwise in the intermediate transitions the colours will be irregularly disposed.

5. A plate of rock-crystal cut perpendicular to the axis loses its successive polarization, and behaves exactly as an ordinary crystallized film through which rectilinear polarized light is transmitted.

6. A thick plate of unannealed glass undergoes a series of regular transformations, the principal phases of which are shown, fig. 5.

V.

The phenomena of successive or rotatory polarization I have experimentally demonstrated admit of a very simple explanation.

The polarized light incident on the crystallized plate is resolved into two portions of equal intensity, polarized at right angles to each other, one in the principal section, the other perpendicular thereto. These resolved portions, when they fall on the silver plate, have their planes of polarization each at an azimuth of 45° , one to the right, the other to the left of the plane of reflection. These are again resolved in the plane of reflection and the plane perpendicular thereto, and are, in consequence of the unequal retardation, which in silver at an angle of 72° amounts to a quarter of an undulation, converted into circularly polarized beams, one right-handed, the other left-handed.

The various homogeneous rays being accelerated differently in their transmission through the two sections of the crystallized plate, this difference is preserved after reflection from the silver plate, and the oppositely circularly polarized beams are reflected with the same difference of phase as the two plane-polarized rays are when emerging from the crystallized lamina. The composition of two circular waves, one right-handed, the other left-handed, gives for resultant a plane wave the azimuth of which varies with the difference of phase of the two components.

When the plane of polarization does not lie equally between the two rectangular sections of the laminæ, these still remaining 45° from the plane of reflection of the silver plate, the beam is resolved into two unequal portions, the amplitudes of which are as $\sin \alpha$ to $\cos \alpha$. Each therefore gives rise to a circular undulation of different amplitude. The resultant of two opposite circular undulations of different amplitudes is an ellipse of constant form, the axes of which vary in position according to the difference of phase. The same phenomena of successive polarization are therefore exhibited, in whatever azimuth the lamina is turned in its own plane; but the tints become fainter and fainter until ultimately, when the principal or perpendicular section is parallel to the plane of reflection of the polarizing plate, all colour disappears.

VI.

By means of the phenomena of successive polarization it is easy to determine which is the thicker of two films of the same crystalline substance. Place one of the films on the diaphragm (a) of the instrument (fig. 1*a*) in the position to show, say, right-handed polarization, then cross it with the other film; if the former be the thicker, the successive polarization will be still right-handed; if both be equal there will be no polarization; and if the crossed film be the thicker, the successive polarization will be left-handed. In this manner a series of films may be readily arranged in their proper order in the scale of tints.

VII.

In the experiments I have previously described the planes of reflection of the polarizing mirror and of the silver plate were coincident; some of the results obtained when the azimuth of the plane of reflection of the silver plate is changed are interesting.

I will confine my attention here to what takes place when the plane of reflection of the silver plate is 45° from that of the polarizing reflector.

When the principal sections of the film are parallel and perpendicular to the plane of reflection of the polarizing mirror, as the whole of the polarized light passes through one of the sections, no interference can take place, and no colour will be seen, whatever be the position of the analyzer.

When the principal sections of the film are parallel and perpendicular

to the plane of reflection of the silver plate, they are 45° from the plane of reflection of the polarizing mirror.

The polarized ray is then resolved into two components polarized at right angles to each other; one component is polarized in the plane of reflection of the silver plate, the other perpendicular thereto; and one is retarded upon the other by a quarter of an undulation.

When the analyzer is at 0° or 90° no colours are seen, because there is no interference; but when it is placed at 45° or 135° , interference takes place, and the same colour is seen as if light circularly polarized had been passed through the film. The bisected and inverted film (fig. 4) shows simultaneously the two complementary colours.

But when the film is placed with one of its principal sections $22\frac{1}{2}^\circ$ from the plane of reflection of the polarizing mirror, on turning round the analyzer the appearances of successive polarization are reproduced exactly as when the planes of reflection of the silver plate and of the polarizing mirror coincide. In this case the components of the light oppositely polarized in the two sections are unequal, being as $\cos 22\frac{1}{2}^\circ$ to $\sin 22\frac{1}{2}^\circ$; these components respectively fall $22\frac{1}{2}^\circ$ from the plane of reflection of the silver plate and from the perpendicular plane, and are each resolved in the same proportion in these two planes. The weak component of the first, and the strong component of the second, are resolved into the normal plane, while the strong component of the first and the weak component of the second are resolved into the perpendicular plane.

VIII.

As bearing intimately on the subject of this paper, I will here quote a passage from a memoir presented by Fresnel to the French Academy of Sciences in 1817, and published, in abstract, in the ‘*Annales de Chimie*,’ t. xxviii. 1825:—

“If a thin crystallized plate be placed between two parallelopipeds of glass crossed at right angles, in each of which the light previously polarized undergoes two total reflections at the incidence of $54\frac{1}{2}^\circ$, first before its entrance into the plate (which we suppose perpendicular to the rays), and subsequently after its emergence, and if, besides, the plate be turned so that its axis makes an angle of 45° with the two planes of double reflection, this system will present the optical properties of plates of rock-crystal perpendicular to the axis, and of liquids which colour polarized light. When the principal section of the rhomboid with which the emergent light is analyzed is turned round, the two images will gradually change colour, instead of experiencing only simple variations in the vividness of their tints, as occurs in the ordinary case of thin crystallized plates; besides, the nature of these colours depends only on the respective inclination of the primitive plane of polarization and the principal section of the rhomboid—that is to say, of the two extreme planes of polarization; thus, when this angle remains constant, the system of the crystallized

plate and the two parallelopipeds may be turned round the transmitted pencil without changing the colour of the images. It is this analogy between the optical properties of this little apparatus and those of plates of rock-crystal perpendicular to the axis which enabled M. Fresnel to foresee the peculiar characters of double refraction that rock-crystal exerts on rays parallel to the axis."

It does not appear that Fresnel, in any of his published memoirs, has given any further modifications of this experiment, the importance of which has been almost entirely overlooked in elementary treatises on light. He does not seem to have remarked that similar phenomena of successive polarization are exhibited when the light incident on the crystallized plate is plane-polarized, nor that the order of the succession of the colours depends on the position of the principal section with respect to the plane of polarization. These circumstances are indeed necessarily included in the beautiful theory established by this eminent philosopher; but I am not aware that they have hitherto been specifically deduced or experimentally shown.

IX.

The apparatus (fig. 1) affords also the means of obtaining large surfaces of uncoloured or coloured light in every state of polarization—rectilinear, elliptical, or circular.

It is for this purpose much more convenient than a Fresnel's rhomb, with which but a very small field of view can be obtained. It must, however, be borne in mind that the circular and elliptical undulations are inverted in the two methods: in the former case they undergo only a single, in the latter case a double reflection.

For the experiments which follow, the crystallized plate must be placed on the diaphragm E between the silver plate and the analyzer, instead of, as in the preceding experiments, between the polarizer and the silver plate.

By means of a moving ring within the graduated circle D the silver plate is caused to turn round the reflected ray, so that, while the plane of polarization of the ray remains always in the plane of reflection of the glass plate, it may assume every azimuthal position with respect to the plane of reflection of the silver plate. The film to be examined and the analyzer move consentaneously with the silver plate, while the polarizing mirror remains fixed.

In the normal position of the instrument the ray polarized by the mirror is reflected unaltered by the silver plate; but when the ring is turned to 45° , 135° , 225° , or 315° , the plane of polarization of the ray falls 45° on one side of the plane of reflection of the silver plate, and the ray is resolved into two others, polarized respectively in the plane of reflection and the perpendicular plane, one of which is retarded on the other by a quarter of an undulation, and consequently gives rise to a circular ray, which is right-handed or left-handed according to whether the ring is turned

45° and 225° or 135° and 315° . When the ring is turned so as to place the plane of polarization in any intermediate position between those producing rectilinear and circular light, elliptical light is obtained, on account of the unequal resolution of the ray into its two rectangular components.

Turning the ring of the graduated diaphragm from left to right when the crystallized film is between the silver plate and the analyzer, occasions the same succession of colours for the same angular rotation as rotating the analyzer from right to left when the instrument is in its normal position and the film is between the polarizer and the silver plate.

X.

To arrange the apparatus for the ordinary experiments of plane-polarized light without the intervention of the silver plate, all that is necessary is to remove the silver plate from the frame F, and to substitute for it a plate of black glass, which must be fixed at the proper polarizing-angle.

To convert it into a Norrenberg's polarizer, a silvered mirror must be laid horizontal at H, and the instrument straightened, as shown at fig. 3, so that a line perpendicular to the mirror shall correspond with the line of sight. The silver plate must be removed from the frame F, and a plate of transparent glass substituted for it, which must be so inclined that the light falling upon it shall be reflected at the polarizing-angle perpendicularly toward the horizontal mirror. The eye will receive the polarized ray reflected from the mirror; and the polarized ray will have passed, before it reaches the eye, twice through a crystallized plate placed between the mirror and the polarizer. The result is the same as if, in the ordinary apparatus, the polarized ray had passed through a plate of double the thickness.

Fig. 2 shows the addition to the apparatus when the coloured rings of crystals are to be examined by light circularly or elliptically polarized: *a* is the optical tube containing the lenses (which require no particular explanation), and *b* the condenser, over which the plate is to be placed.

II. "On an approximately Decennial Variation of the Temperature at the Observatory at the Cape of Good Hope between the years 1841 and 1870, viewed in connexion with the Variation of the Solar Spots." By E. J. STONE, F.R.S., Astronomer Royal at the Cape of Good Hope. In a Letter to the President. Received February 21, 1871.

Royal Observatory, Cape of Good Hope, Jan. 17, 1871.

DEAR SIR,—I enclose a curve of the variation of the annual mean temperature at the Cape deduced from observations extending from 1841 to 1870 inclusive. I have carefully examined the zero-points of all the ther-

mometers which have been employed in this series of observations. I have then deduced the rate of change of these thermometers, from a comparison of the index-errors thus found and those given originally or obtained in 1852 by Sir Thomas Maclear, when he compared the principal thermometers at the Observatory with the readings of a standard "Regnault" which had been sent out to the Observatory for that purpose by you. These indications of change have been carefully checked by all the comparisons made, at different times and for different purposes, of these thermometers *inter se* and with others which still remain at the Observatory. From the agreement of the different results thus checked, I have no doubt upon my own mind of the systematic character and sensible amount of the increase of readings of thermometers with age thus indicated. In some cases the change appears to amount to as much as $0^{\circ}05$ F. per annum. From these results I have deduced the index-errors of the different thermometers for the different periods, and applied these corrections throughout. I have also corrected the mean results of the five observations made daily since 1847 in order to deduce the true daily mean.

The results thus reduced on a general system, and extending over thirty years, appeared likely to afford information respecting any connexion which might exist between the mean temperature and the frequency of solar spots. I have therefore constructed the curves of variation of mean annual temperature, and the inverse curve of solar-spot frequency for comparison. The latter curve has been founded upon Wolf's observations.

The observations of temperature from 1841 to 1851 inclusive were made in the original Meteorological Observatory, which was burnt down in 1852, March 11.

The observations from 1852, April 24, to 1858, August 31, were made in a wooden shed erected for the purpose on the site of the old Observatory.

The observations from 1858, August 31st, to the present time have been made in the crib before the south-west window of the Transit-Circle Room.

These changes are so far unfortunate that there is clearly a change of mean temperature arising from the different circumstances of exposure. I have therefore referred each set of observations to the mean temperature deduced from all the observations made under the same circumstances of exposure. The deviations of the mean temperature for each year from the mean of the whole period of similar exposure are then laid down as ordinates on the scale of one division of the ruled paper to $0^{\circ}05$ F. To smooth down the irregularities, I have joined the points thus laid down, and bisected the lines thus joining these points whenever the corresponding mean temperatures were deduced from a full year's observations. In other cases the temperatures corresponding to the deficient months have been supplied from the adjoining years, and the resulting mean temperature allowed less weight. The inverse curve of the frequency of solar spots has been formed by simply subtracting 100 from Wolf's numbers, and

both of the solar spots near the minimum, and of the mean temperature also, are taken into account, such discrepancies might perhaps fairly be expected, even if there be a physical connexion between the two phenomena as results of some common cause. If there be a sensible inequality in the mean temperature with a period of about ten years, then the mean temperature resulting from the observations in the temporary Observatory, which were made near a maximum, will be too high. The corresponding ordinates, therefore, will be depressed too much relatively to those corresponding to observations made in the other two observatories. In the curve 2, I have imperfectly corrected the mean of the results for the temporary observatory on the supposition of such an inequality existing. The only result of such a correction is to modify the curve at the points of junction of the observations made in different positions. The general form is unaltered. It should be mentioned that the point about which the curves appear to differ most is near or at the change of exposure from the original observatory to the temporary shed, about 1852.

I may mention that I had not the slightest expectation, on first laying down the curves, of any sensible agreement resulting, but that I now consider the agreement too close to be a matter of chance. I should, however, rather lean to the opinion that the connexion between the variation of mean temperature and the appearance of solar spots is indirect rather than direct, that each results from some general change of solar energy.

I have forwarded these curves to you, knowing the great interest you have ever taken in such inquiries, and on account of your being the chief promoter of the establishment of a Meteorological Observatory here. The problems of meteorology appear to be presented here in a simpler form than in England; and probably systematic photographic self-registering observations extended over a few years might lead to important results.

I have the honour to be, Sir,

Yours obediently,

E. J. STONE.

Sir Edward Sabine, K.C.B., P.R.S., &c.

III. *Résumé* of two Papers on Sun-spots:—"On the Form of the Sun-spot Curve," by Prof. WOLF; and "On the Connexion of Sun-spots with Planetary Configuration," by M. FRITZ. By B. LOEWY. Communicated by WARREN DE LA RUE, F.R.S., and B. STEWART, F.R.S.

Of these two series of investigations, one is by Professor Wolf, the other by M. Fritz, communicated to Wolf.

In the first, Prof. Wolf has proposed to himself to find the mean character of the curve of sun-spots, *i. e.* its real form from one minimum to another. He investigates the form only for $2\frac{1}{2}$ years before, and $2\frac{1}{2}$ years

after each minimum, and concludes by a simple proportion of the remainder. He finds that the curve *ascends* more rapidly than it *descends*—the ascent taking in the mean 3·7 years, the descent lasting 7·4 years. We have established these data far more reliably in our last paper; and our curve gives 3·52 years for the ascent, 7·54 years for the descent (average of the three periods). Professor Wolf also thinks that although a single period may differ essentially in its character and form from the mean, still, on the whole, if the descent is retarded, the ascent in the same period is also retarded; if the former is accelerated, the latter is also accelerated. This is not quite borne out by our curve. He also overlooks the secondary maximum, which may lead to great conclusions if more investigated together with other matters.

M. Fritz comes to the following conclusions:—

1. The connexion between sun-spots and auroral and magnetic disturbances indicates an external cause, to be sought in planetary configurations.

2. The relative influence of the planets must be exerted in the following order:—Jupiter (greatest), Venus, Mercury, Earth, Saturn.

3. This influence cannot entirely depend on the time of rotation; but changes in the *magnetic* axes of these planets may have the most determining effect.

4. Investigating the comparative influences of them singly and together (as far as possible), at the times of conjunction and quadrature, he finds the greatest coincidence of maxima of sun-spots with the time when *Jupiter* and *Saturn* are in quadrature; and the greatest coincidence of minima when these planets are in conjunction.

5. There is also (a minor) coincidence of maxima when Jupiter and Venus are in quadrature.

There is also an extension of the paper for finding the connexions with auroras, and a statement that every 27·7 days there seems to be a monthly maximum, which may probably be explained (according to Fritz) by the tendency of a particular solar meridian to spot-formations, depending upon the presence of an intra-Mercurial planet.

March 30, 1871.

General Sir EDWARD SABINE, K.C.B., President, in the Chair.

The following communications were read:—

- I. “Experiments in Pangenesis, by Breeding from Rabbits of a pure variety, into whose circulation blood taken from other varieties had previously been largely transfused.” By FRANCIS GALTON, F.R.S. Received March 23, 1871.

Darwin's provisional theory of Pangenesis claims our belief on the ground that it is the only theory which explains, by a single law, the numerous

phenomena allied to simple reproduction, such as reversion, growth, and repair of injuries. On the other hand, its postulates are hypothetical and large, so that few naturalists seem willing to grant them. To myself, as a student of Heredity, it seemed of pressing importance that these postulates should be tested. If their truth could be established, the influence of Pangenesis on the study of heredity would be immense; if otherwise the negative conclusion would still be a positive gain.

It is necessary that I should briefly recapitulate the cardinal points of Mr. Darwin's theory. They are (1) that each of the myriad cells in every living body is, to a great extent, an independent organism; (2) that before it is developed, and in all stages of its development, it throws "gemmules" into the circulation, which live there and breed, each truly to its kind, by the process of self-division, and that, consequently, they swarm in the blood, in large numbers of each variety, and circulate freely with it; (3) that the sexual elements consist of organized groups of these gemmules; (4) that the development of certain of the gemmules in the offspring depends on their consecutive union, through their natural affinities, each attaching itself to its predecessor in a regular order of growth; (5) that gemmules of innumerable varieties may be transmitted for an enormous number of generations without being developed into cells, but always ready to become so, as shown by the almost insuperable tendency to feral reversion, in domesticated animals.

It follows from this, and from the general tenor of Mr. Darwin's reasoning and illustrations, that two animals, to outward appearance of the same pure variety, one of which has mongrel ancestry and the other has not, differ solely in the constitution of their blood, so far as concerns those points on which outward appearance depends. The one has none but gemmules of the pure variety circulating in his veins, and will breed true to his kind; the other, although only the pure variety of skin-gemmules happens to have been developed in his own skin, has abundance of mongrel gemmules in his blood, and will be apt to breed mongrels. It also follows from this that the main stream of heredity must flow in a far smaller volume from the developed parental cells, of which there is only one of each variety, than from the free gemmules circulating with the blood, of which there is a large number of each variety. If a parental developed cell bred faster than a free gemmule, an influx of new immigrants would gradually supplant the indigenous gemmules; under which supposition, a rabbit which, at the age of six months, produced young which reverted to ancestral peculiarities, would, when five years old, breed truly to his individual peculiarities; but of this there is no evidence whatever.

Under Mr. Darwin's theory, the gemmules in each individual must therefore be looked upon as entozoa of his blood, and, so far as the problems of heredity are concerned, the body need be looked upon as little more than a case which encloses them, built up through the development of some of their number. Its influence upon them can be only such as

would account for the very minute effects of use or disuse of parts, and of acquired mental habits being transmitted hereditarily.

It occurred to me, when considering these theories, that the truth of Pangenesis admitted of a direct and certain test. I knew that the operation of transfusion of blood had been frequently practised with success on men as well as animals, and that it was not a cruel operation—that not only had it been used in midwifery practice, but that large quantities of saline water had been injected into the veins of patients suffering under cholera. I therefore determined to inject alien blood into the circulation of pure varieties of animals (of course, under the influence of anæsthetics), and to breed from them, and to note whether their offspring did or did not show signs of mongrelism. If Pangenesis were true, according to the interpretation which I have put upon it, the results would be startling in their novelty, and of no small practical use; for it would become possible to modify varieties of animals, by introducing slight dashes of new blood, in ways important to breeders. Thus, supposing a small infusion of bull-dog blood was wanted in a breed of greyhounds, this, or any more complicated admixture, might be effected (possibly by operating through the umbilical cord of a newly born animal) in a single generation.

I have now made experiments of transfusion and cross circulation on a large scale in rabbits, and have arrived at definite results, negating, in my opinion, beyond all doubt, the truth of the doctrine of Pangenesis.

The course of my experiments was as follows:—Towards the end of 1869, I wrote to Dr. Sclater, the Secretary of the Zoological Society, explaining what I proposed to do, and asking if I might be allowed to keep my rabbits in some unused part of the Gardens, because I had no accommodation for them in my own house, and I was also anxious to obtain the skilled advice of Mr. Bartlett, the Superintendent of the Gardens, as to their breed and the value of my results. I further asked to be permitted to avail myself of the services of their then Prosector, Dr. Murie, to make the operations, whose skill and long experience in minute dissection is well known. I have warmly to thank Dr. Sclater for the large assistance he has rendered to me, in granting all I asked, to the full, and more than to the full; and I have especially to express my obligations to the laborious and kind aid given to me by Dr. Murie, at real inconvenience to himself, for he had little leisure to spare. The whole of the operations of transfusion into the jugular vein were performed by him, with the help of Mr. Oscar Fraser, then Assistant Prosector, and now appointed Osteologist to the Museum at Calcutta, I doing no more than preparing the blood derived from the supply-animal, performing the actual injection, and taking notes. The final series of operations, consisting of cross-circulation between the carotid arteries of two varieties of rabbits, took place after Dr. Murie had ceased to be Prosector. They were performed by Mr. Oscar Fraser in a most skilful manner, though he and I were still further indebted, on more than one occasion, to Dr. Murie's advice and assistance. My part in this series was limited to in-

serting and tying the canulæ, to making the cross-connexions, to recording the quality of the pulse through the exposed arteries, and making the other necessary notes.

The breed of rabbits which I endeavoured to mongrelize was the "Silver-grey." I did so by infusing blood into their circulation, which I had previously drawn from other sorts of rabbits, such as I could, from time to time, most readily procure. I need hardly describe Silver-grey rabbits with minuteness. They are peculiar in appearance, owing to the intimate mixture of black and grey hairs with which they are covered. They are never blotched, except in the one peculiar way I shall shortly describe; and they have never lop ears. They are born quite black, and their hair begins to turn grey when a few weeks old. The variations to which the breed is liable, and which might at first be thought due to mongrelism, are white tips to the nose and feet, and also a thin white streak down the forehead. But these variations lead to no uncertainty, especially as the white streak lessens or disappears, and the white tips become less marked, as the animal grows up. Another variation is much more peculiar: it is the tendency of some breeds to throw "Himalayas," or white rabbits with black tips. From first to last I have not been troubled with white Himalayas; but in one of the two breeds which I have used, and which I keep carefully separated from each other, there is a tendency to throw "sandy" Himalayas. One of these was born a few days after I received the animals, before any operation had been made upon them, and put me on my guard. A similar one has been born since an operation. Bearing these few well-marked exceptions in mind, the Silver-grey rabbit is excellently adapted for breeding-experiments. If it is crossed with other rabbits, the offspring betray mongrelism in the highest degree, because any blotch of white or of colour, which is not "Himalayan," is almost certainly due to mongrelism; and so also is any decided change in the shape of the ears.

I shall speak in this memoir of litters connected with twenty silver-grey rabbits, of which twelve are does and eight are bucks; and eighteen of them have been submitted to one or two of three sorts of operations. These consisted of:—

(1) Moderate transfusion of partially defibrinized blood. The silver-grey was bled as much as he could easily bear; that was to about an ounce, a quantity which bears the same proportion to the weight of his body (say 76 oz.) that 2 lbs. bears to the weight of the body of a man (say 154 lbs.); and the same amount of partially defibrinized blood, taken from a killed animal of another variety, was thrown in in its place. The blood was obtained from a yellow, common grey, or black and white rabbit, killed by dividing the throat, and received in a warmed basin, where it was stirred with a split stick to remove part of the fibrine. Then it was filtered through linen into a measuring-glass, and thence drawn up with a syringe, graduated into drachms; and the quantity injected was noted.

(2) The second set of operations consisted in a large transfusion of wholly

defibrinized blood, which I procured by whipping it up thoroughly with a whisk of rice-straw ; and, in order to procure sufficient blood, I had on one occasion to kill three rabbits. I alternately bled the silver-grey and injected, until in some cases a total of more than 3 ounces had been taken out and the same quantity, wholly defibrinized, had been thrown in. This proportion corresponds to more than 6 lbs. of blood in the case of a man.

(3) The third operation consisted in establishing a system of cross-circulation between the carotid artery of a silver-grey and that of a common rabbit. It was effected on the same principle as that described by Addison and Morgan (*Essay on Operation of Poisonous Agents upon the Living Body*. Longman & Co., 1829), but with more delicate apparatus and for a much longer period. The rabbits were placed breast to breast, in each other's arms, so that their throats could be brought close together. A carotid of each was then exposed ; the circulation in each vessel was temporarily stopped, above and below, by spring holders ; the vessels were divided, and short canulæ, whose bores were larger than the bore of the artery in its normal state, were pressed into the mechanically distended mouths of the arteries ; the canulæ were connected cross-wise ; the four spring holders were released, and the carotid of either animal poured its blood direct into the other. The operation was complicated, owing to the number of instruments employed ; but I suspended them from strings running over notched bars, with buttous as counterpoises, and so avoided entanglement. These operations were exceedingly successful ; the pulse bounded through the canulæ with full force ; and though, in most cases, it began to fall off after ten minutes or so, and I was obliged to replace the holders, disconnect the canulæ, extract the clot from inside them with a miniature corkscrew, reconnect the canulæ, and reestablish the cross-flow two, three, or more times in the course of a single operation, yet on two occasions the flow was uninterrupted from beginning to end. The buck rabbit, which I indicate by the letter O, was $37\frac{1}{2}$ minutes in the most free cross-circulation imaginable with his "blood-mate," a large yellow rabbit. There is no mistaking the quality of the circulation in a bared artery ; for, when the flow is perfectly free, the pulse throbs and bounds between the finger and thumb with a rush, of which the pulse at the human wrist, felt in the ordinary way, gives an imperfect conception.

These, then, are the three sorts of operations which I have performed on the rabbits ; it is convenient that I should distinguish them by letters. I will therefore call the operation of simply bleeding once, and then injecting, by the letter α ; that of repeated bleedings and repeated injections by the letter ω ; and that of cross-circulation by the letter x .

In none of these operations did I use any chemical means to determine the degree to which the blood was changed ; for I did not venture to compromise my chances of success by so severe a measure ; but I adopted the following method of calculation instead :—

I calculate the change of blood effected by transfusion, or by cross-circulation, upon moderate suppositions as to the three following matters:—

- (1) The quantity of blood in a rabbit of known weight.
- (2) The time which elapses before each unit of incoming blood is well mixed up with that already in the animal's body.
- (3) The time occupied by the flow, through either carotid, of a volume of blood equal to the whole contents of the circulation.

As regards 1, the quantity of blood in an animal's body does not admit, by any known method, of being accurately determined. I am content to take the modern rough estimate, that it amounts to one-tenth of its total weight. If any should consider this too little, and prefer the largest estimate, viz. that in Valentin's 'Repertorium,' vol. iii. (1838), p. 281, where it is given for a rabbit as one part in every 6·2 of the entire weight, he will find the part of my argument which is based on transfusion to be weakened, but not overthrown, while that which relies on cross-circulation is not sensibly affected.

As regards 2, the actual conditions are exceedingly complex; but we may evade their difficulty by adopting a limiting value. It is clear that when only a brief interval elapses before each unit of newly infused blood is mixed with that already in circulation, the quality of the blood which, at the moment of infusion into one of the cut ends of the artery or vein, is flowing out of the other, will be more alienized than if the interval were longer. It follows that the blood of the two animals will intermix more slowly when the interval is brief than when it is long. Now I propose to adopt an extreme supposition, and to consider them to mix *instantaneously*. The results I shall thereby obtain will necessarily be less favourable to change than the reality, and will protect me from the charge of exaggerating the completeness of intermixture.

As regards 3, I estimate the flow of blood through either carotid to be such that the volume which passes through it in ten minutes equals the whole volume of blood in the body. This is a liberal estimate; but I could afford to make it twice or even thrice as liberal, without prejudice to my conclusions.

Upon the foregoing data the following Table has been constructed. The formulæ are:—Let the blood in the Silver-grey be called a , and let its volume be V , and let the quantity u of alien blood be thrown in at each injection, then the quantity of blood a remaining in the Silver-grey's circulation, after n injections,

$$= V \left(1 - \frac{u}{V} \right)^n.$$

If the successive injections be numerous and small, so as to be equivalent to a continuous flow, then, after w of alien blood has passed in, the formula becomes $V \cdot e^{-\frac{w}{V}}$.

A comparison of the numerical results from these two formulæ shows that no sensible difference is made if (within practicable limits) few and large, or many and small, injections are made, the total quantity injected being the same.

In cross-circulation the general formula is this :—If V' be the volume of blood in the other rabbit, after w of alien blood has passed through either canula, the quantity of blood a remaining in the Silver-grey exceeds *

$\frac{V}{V+V'} \left\{ V + V' e^{-\left(\frac{1}{V} + \frac{1}{V'}\right)w} \right\}$. This becomes $\frac{V}{2} \left\{ 1 + e^{-\frac{2w}{V}} \right\}$ when $V=V'$;

also, when V' is infinite, it gives the formula already mentioned for injection by a continuous flow of purely alien blood.

TABLE I.
(Contents of circulation of Silver-grey Rabbit=100.)

Quantity of blood infused.	Maximum percentage of original blood remaining after					Period, in minutes, during which the continuous flow through each carotid has lasted.
	Successive injections of purely alien blood, each = $\frac{100}{12}$.		Continuous flow of purely alien blood.	Cross-circulation.		
				Rabbits of equal size.	Blood-mate $\frac{1}{10}$ larger than the Silver-grey.	
25	3	77	78	80	80	2½
50	6	59	61	68	68	5
75	9	46	47	61	60	7½
100	12	35	37	56	55	10
125	15	27	29	54	52	12½
150	18	21	22	52	51	15
175	21	16	17	51	50	17½
200	24	12	14	51	49	20
300	36	4	5	50	48	30
400	48	1	2	50	48	40
infinite	infinite	0	0	50	48	infinite.

I now give a list (Table II.) of the rabbits to which, or to whose blood-mates, I shall have to refer. Every necessary particular will be found in the Table :—the weight of the rabbits; the estimated weight of blood in their veins; the operations performed on them, whether u , w , or x ; the particulars of those several operations; the estimated percentage of alien blood that was substituted for their natural blood; and lastly, the colour, size, and breed of their blood-mates.

* I am indebted to Mr. George Darwin for this formula.
2 R 2

TABLE II.

Silver-grey Does.	Weight of rabbit.	Estimated weight of blood.	Nature of operation*.	Drachms infused, and period of cross-circulation.	Percentage of alienized blood.	Colour &c. of blood-mate.
	lbs. oz.	drachms.				
A	5 9	79	u	9	11	{ Common grey and white.
			{ u	10	12	Yellow, large.
B	5 13	82	{ x	{ 10 min. perfect, 15 or 20 very good.	{ 50, or more.	{ Common grey.
C	5 8	78	u	9.5	12	Albino, large.
D	5 4	75	u	8.5	12	Himalaya.
			{ u	8	14	Common grey.
E	4 9	58	{ x	{ 13 min. good, 14 poor.	{ 50, about	Common grey.
F	4 13	61	u	7.7	10	{ Black and white, large.
			{ w	{ 25.5, in 6 injections.	{ 35	{ Grey and black, speckled.
G	4 11	60	{ x	{ 31 min. good, total.	{ 75	Common grey.
H	x	{ 15 min. perfect, 15 very good.	{ 50	Common grey.
I†	x	{ 16 min. perfect, not much more.	{ nearly 50	{ Common grey and white.
J†	x	35 min. perfect.	...	{ Yellow, brown mouth (? Himalaya).
S	x	{ too unsuccessful to be worth counting.	{ ? any.	{ Angora, fawn and white.
T	None.	None.
Bucks.						
K	4 14	62	{ u	9	14	{ Yellow, brown mouth.
			{ w	{ 14, in 4 injections, total.	{ 32	Yellow and white.
L	4 13	61	u	7	11	Common grey.
			{ u	7	14	Black and white.
M	4 0	51	{ w	{ 24.5, in 6 injections, total.	{ 45	{ 3 black and white in succession.
			{ u	7.5	13	{ Angora, grey and white, red eyes.
N	4 9	58	{ w	{ 16.5, in 4 injections, total.	{ 34	Yellow.
O (son of C (u) by K (u))	{ ...	{ ...	{ x	{ 37½ min. perfect.	{ 50	Yellow.
P†	x	{ 25 to 30 min. perfect.	{ 50	Common grey.
Q†	x	{ 15 min. perfect, 15 very good.	{ 50	Yellow and white.
	x	{ 25 min. pretty good.	{ 50	{ Common grey and white.

* *Note* (to 4th column).—*u* means simple transfusion, by one copious bleeding, and then injecting; *w* means compound transfusion by successive bleedings and successive injections; *x* means cross-circulation.

† These rabbits belong to a breed liable to throw "Sandy" Himalayas.

TABLE III.

Litters subsequent to first transfusion. Both parents Silver-greys. Average proportion of alienized blood in either parent = $\frac{1}{8}$; therefore in young $\frac{1}{8}$ also.

Out of	By	Number and character of litters.
A	K	4 true Silver-greys.
A	M	5 ditto, but 1 had a white foot to above knee.
B	K	5 true Silver-greys.
C	K	6 ditto.
D	K	4 ditto.
E	L	6 ditto.
		—
		30 all true Silver-greys, except possibly one instance.

Litters subsequent to second transfusion of buck. Both parents Silver-greys. Average proportion of alienized blood in young about $\frac{1}{4}$.

Out of	By	Number and character of litters.
A	M	6 true Silver-greys.

Litters subsequent to cross-circulation of buck only, the does being O or u. Both parents Silver-greys. Average proportion of blood in young between $\frac{1}{4}$ and $\frac{1}{8}$.

Out of	By	Number and character of litters.
S	O	5 true Silver-greys.
C	O	5 ditto.
T	O	3 ditto.
		—
		13 all Silver-greys.

Litters subsequent to cross-circulation of both parents (Silver-greys). Average proportion of alienized blood in young fully $\frac{1}{2}$.

Out of	By	Number and character of litters.
B	O	3 true Silver-greys.
H	O	7 ditto.
H	O	7 ditto.
I*	P*	6 ditto.
J*	Q*	6 ditto, all but one, a sandy Himalaya.
J*	P*	8 true Silver-greys.
		—
		37 36 Silver-greys, 1 Himalaya.

* These rabbits belong to a breed liable to throw "Sandy" Himalayas.

Litters subsequent to cross-circulation of both parents (common rabbits).

Average proportion of alienized blood in young a little less than $\frac{1}{2}$.

Out of blood-mate to	By blood-mate to	Number and character of litters.
E	R	8 none Silver-grey, all like father or mother.
E	Q*	5 ditto.
G	O	9 ditto.
I*	Q*	8 ditto.
J*	Q*	8 ditto.
		—
		38 none Silver-greys.

In another list (Table III.) I give particulars of all the litters I have obtained from these rabbits, classified according to the operations which the parents had previously undergone.

I will now summarize the results. In the first instance I obtained five does (A, B, C, D, and E) and three bucks (K, L, and M) which had undergone the operation which I call u , and which had in consequence about $\frac{1}{2}$ of their blood alienized. I bred from these †, partly to see if I had produced any effect by the little I had done, and chiefly to obtain a stock of young rabbits which would be born with $\frac{1}{2}$ of alien gemmules in their veins, and which, when operated upon themselves, would produce descendants having nearly $\frac{1}{4}$ alienized blood (the exact proportion is $1 - (1 - \frac{1}{2})^2 = \frac{1}{4}$). I obtained thirty young ones in six litters; and they were all true silver-greys, except, possibly, in one instance (out of the doe A (u) by the buck M (u)), where one, of a litter of five, had a white fore leg, the white extending to above the knee-joint. This white leg gave me great hopes that Pangenesis would turn out to be true, though it might easily be accounted for by other causes; for my stock were sickly (both those on which I had not operated and those on which I had suffering severely from a skin disease), and it was natural under those circumstances of ill health that more white than usual should appear in the young.

Having, then, had experience in transfusion, and feeling myself capable of managing a more complicated operation without confusion, I began the series which I call w . I left my old lot of does untouched, but obtained one new doe (G(w)), which had undergone the last operation, and three bucks (K (u, w), M (u, w), N (u, w)) which had undergone both operations, u and w . On endeavouring to breed from them, the result was unexpected, they appeared to have become sterile. The bucks were as eager as possible for the does; but the latter proving indifferent, I was unable to testify to their union having taken place; so I left them in pairs, in the same hutch, for periods of three days at a time. Attempts were made in this

* These rabbits belong to a breed liable to throw "Sandy" Himalayas.

† I always allowed the bucks to run for awhile with waste does before commencing the breeding-experiments, that all old reproductive material might be got rid of.

way, to breed from them in seven instances; and five of them were utter failures. One case was quite successful; and that, fortunately, was of the same pair ($A(u)$ and $M(u, w)$) which, under the u operation, had bred the white-footed young one. This time, the offspring (six in number) were pure silver-greys. The last case was unfortunate. The doe ($E(u)$) had been once sterile to its partner ($N(u, w)$), and she had been put again in the same hutch with him for a short period, but was thought not to have taken him. She was shortly afterwards submitted to the operation x . From this she had nearly recovered when she brought forth an aborted litter and died. I was absent from town at the time; but Mr. Fraser, who examined them, wrote to say he fully believed that some were pied; if so, it must have been under the influence of the cross-circulation. But I have little faith in the appearance of the skin of naked, immature rabbits; for I have noticed that difference of transparency, and the colour of underlying tissues, give fallacious indications.

My results thus far came to this, viz. that by injecting defibrinized blood I had produced no other effect than temporary sterility. If the sterility were due to this cause alone, my results admitted of being interpreted in a sense favourable to Pangenesis, because I had deprived the rabbits of a large part of that very component of the blood on which the restoration of tissues depends, and therefore of that part in which, according to Pangenesis, the reproductive elements might be expected to reside. I had injected alien corpuscles but not alien gemmules. The possible success of the white foot, in my first litters, was not contradicted by the absence of any thing of the sort in my second set, because the additional blood I had thrown in was completely defibrinized. It was essential to the solution of the problem, that blood in its natural state should be injected; and I thought the most convenient way of doing so was by establishing cross-circulation between the carotids. If the results were affirmative to the truth of Pangenesis, then my first experiments would not be thrown away; for (supposing them to be confirmed by larger experience) they would prove that the reproductive elements lay in the fibrine. But if cross-circulation gave a negative reply, it would be clear that the white foot was an accident of no importance to the theory of Pangenesis, and that the sterility need not be ascribed to the loss of hereditary gemmules, but to abnormal health, due to defibrinization and perhaps to other causes also.

My operations of cross-circulation (which I call x) put me in possession of three excellent silver-grey bucks, four excellent silver-grey does, and one doe whose operation was not successful enough for me to care to count it. One of my x does (B) had already undergone the operation u , and I had another of my old lot ($C(u)$), which I left untouched. There were also three common rabbits, bucks, which were blood-mates to silver-greys, and four common rabbits, does, also blood-mates of silver-greys. From this large stock I have bred eighty-eight rabbits in thirteen litters, and in no single case has there been any evidence of alteration of breed. There

has been one instance of a sandy Himalaya; but the owner of this breed assures me they are liable to throw them, and, as a matter of fact, as I have already stated, one of the does he sent me, did litter and throw one a few days after she reached me. The conclusion from this large series of experiments is not to be avoided, that the doctrine of Pangenesis, pure and simple, as I have interpreted it, is incorrect.

Let us consider what were the alternatives before us. It seems *à priori* that, if the reproductive elements do not depend on the body and blood together, they must reside either in the solid structure of the gland, whence they are set free by an ordinary process of growth, the blood merely affording nutriment to that growth, or else that they reside in the blood itself. My experiments show that they are not independent residents in the blood, in the way that Pangenesis asserts; but they prove nothing against the possibility of their being temporary inhabitants of it, given off by existing cells, either in a fully developed state or else in one so rudimentary that we could only ascertain their existence by inference. In this latter case, the transfused gemmules would have perished, just like the blood-corpuscles, long before the period had elapsed when the animals had recovered from the operations.

I trust that those who may verify my results will turn their attention to the latter possibility, and will try to get the male rabbits to couple immediately, and on successive days, after they have been operated on. This might be accomplished if there were does at hand ready to take them; because it often happens that when the rabbits are released from the operating-table, they are little, if at all, dashed in their spirits; they play, sniff about, are ready to fight, and, I have no doubt, to couple. Whether after their wounds had begun to inflame, they would still take to the does, I cannot say; but they sometimes remain so brisk, that it is probable that in those cases they would do so. If this experiment succeeded, it would partly confirm the very doubtful case of the pied young of the doe which died after an operation of cross-circulation (which, however, further implies that though the ovum was detached, it was still possible for the mother gemmules to influence it), and it would prove that the reproductive elements were drawn from the blood, but that they had only a transient existence in it, and were continually renewed by fresh arrivals derived from the framework of the body. It would be exceedingly instructive, supposing the experiment to give affirmative results, to notice the gradually waning powers of producing mongrel offspring.

APPENDIX I.

It is important that I should give details of the operations of cross-circulation. I may mention that, having to deal with many rabbits, I distinguished them permanently by tattooing bold Roman numerals in the inside of their ears.

I. Experiments of cross-circulation on one buck and two does, pure silver-greys, of a breed obtained from Mr. E. Royds, of Greenhill, Rochdale, the same breed as that on which all my *u* and *w* experiments had been made.

Oct. 19, 1870.—*Silver-grey buck*, O, out of doe A (*u*) by M (*u*), and therefore own brother to the white-footed young one, a small rabbit, just six months old. His blood-mate was a

Yellow buck, lop-eared, white throat, probably one-fifth heavier than the silver-grey. I avoided unnecessary weighing, because it frightens the animals, and tends to interfere with the final success. At 12^h 30^m I made cross-circulation; flow was perfect; 12^h 35^m, continued perfect; 12^h 40^m, perfect, but yellow to silver-grey perhaps the stronger; 12^h 44^m, ditto; 12^h 50^m, perfect both ways; 12^h 55^m, ditto; 1^h, ditto; 1^h 5^m, ditto; 1^h 7½^m, ditto. I then stopped and tied up. I tested the flow with a small and delicate but very simple pulse-meter on all these occasions, not liking to interfere overmuch with my fingers. I, however, used them at the commencement, at 12^h 50^m, and at 1^h 5^m.

Oct. 20, 1870.—*Silver-grey doe*, B (*u*), a fine large animal; her blood-mate was a *Common large grey lop-eared doe*, about one-tenth heavier than the silver-grey.

1^h, cross-circulation established, apparently perfect; I mean the throbbing of the canula and artery were obvious; 1^h 6^m, felt and found the flow quite good; 1^h 12^m, common to silver-grey quite good, *vice versa* poor; 1^h 15^m, ditto; I disconnected and cleaned and removed clots and reconnected. This I repeated several times; there was still much trouble in maintaining a proper flow from silver to common grey, but common to silver was always good. The operation continued till 1^h 40^m; then I disconnected; and as the silver-grey had received too much, I let her bleed to 4 drachms.

Oct. 27, 1870.—*Silver-grey doe*, H, moderate size; her blood-mate was a *Common large grey doe*, certainly more than a tenth heavier than the silver-grey. There was some trouble with her, as the carotid was abnormal, and three offshoots from it had to be tied before the canula could be inserted.

12^h 48^m, cross-circulation established, perfect pulse, but silver to common the fullest; 12^h 53^m, perfect; 1^h, silver to common perfect, *vice versa* rather poor; 1^h 2^m, ditto; 1^h 7^m, common to silver stopped; I disconnected and cleaned and reconnected, and by 1^h 12^m had reestablished perfect cross-circulation; at 1^h 30^m I had stopped silver to common and made common to silver better; got five minutes good flow, then repeated cleanings and got three minutes more. My estimate at the close of the operation was that the silver-grey gave blood freely for thirty-five minutes, and received it freely for about the same time.

II. Experiments of cross-circulation on two bucks and two does of a silver-grey breed, reputed pure, and looking well-bred animals, but liable to

show russet marks. They were procured of Mr. Vipian, of March, Cambridgeshire, and are of the same breed as those on which Mr. Bartlett made his well-known experiments about the production of Himalayas (Proc. Zool. Soc. 1861). They are liable to throw "Sandy Himalayas," as I found myself, as Mr. Bartlett also found, and as Mr. Vipian informs me is the case. I distinguish this breed by asterisks (*).

Oct. 6, 1870.—*Silver-grey buck*, P*, moderate size; his blood-mate was a *Common grey buck*, with some russet on his back and white on his belly; he was the larger of the two animals.

12^h 50^m, cross-circulation established, perfect; 12^h 55^m, ditto, but silver to common, I think, a trifle the stronger; 12^h 59^m, ditto; 1^h 5^m, common to silver very faint. I stopped them and cleaned out twice and successively; 1^h 15^m, good, but common to silver was the least good; 1^h 25^m, disconnected. My estimate was that there had been an equivalent to fully twenty-five minutes, and perhaps thirty minutes, of capital flow both ways.

Oct. 7, 1870.—*Silver-grey buck*, Q*, moderate size; his blood-mate was a *Yellow buck*, white belly, large.

11^h 40^m, cross-circulation established; 11^h 45^m, quite good; 11^h 50^m, good but not perfect; 11^h 55^m good; 12^h both stopped. Then I made several disconnexions and cleanings, and obtained short periods of success; at 12^h 35^m I finally stopped. My estimate was thirty minutes' good running: the silver-grey received more than his share; there was a slip in the operation, and five drachms of blood were lost between the rabbits; so I did not care to let the silver-grey bleed more.

Oct. 6, 1870.—*Silver-grey doe*, I*, moderate size; her blood-mate was a *Common grey doe*, large.

3^h 40^m, cross-circulation was established; 3^h 44^m, excellent; 3^h 50^m, excellent; 3^h 55^m, excellent; shortly after, something was twisted or otherwise went wrong, and both stopped. I had a good deal of trouble and but little further success. Ten drachms of blood was lost between the rabbits (partly by leakage of the canulæ).

Oct. 7, 1871.—*Silver-grey doe*, J*, moderate size; her blood-mate was a *Yellow doe*, dark about mouth, and also of moderate size. I afterwards became convinced she was simply a sandy Himalaya.

At 2^h 5^m established cross-circulation; 2^h 13^m, quite good; 2^h 20^m, excellent; 2^h 25^m, excellent; 2^h 30^m, ditto; 2^h 35^m, ditto; 2^h 40^m, ditto, then disconnected. An accident occurred at the end, by which the silver-grey lost four drachms of blood.

APPENDIX II.

Description of the method of performing the operations.

It is essential to a fair chance of success that the operator should have a large and thriving stock of full-grown rabbits. They cannot be produced at will in the market; and young ones are so timid and tender that

they are not fit to be operated on. The next essential point is an operating-table, with ample and proper apparatus for holding the rabbits easily but rigidly. It is most improper to subject a helpless animal to an operation without taking every precaution for its success, so as to minimize the necessity for operating. The chief hindrances to success are, entanglement of instruments, or the breaking loose of blood-vessels, both owing to an unexpected start; also an animal will struggle violently, and become terrified if he is loosely held, hoping to get away, whilst if he is firmly secured he lies as though magnetized, without signs of fear or discomfort, and with his pulse and breathing perfectly normal. I regret extremely that, although I took pains to inquire, I did not at first hear of Czermak's recently devised apparatus for holding the head. I began by the old plan of putting the animals in a bag and holding them, which was very unsatisfactory. Then I devised a plan of my own, which was good, but inferior to Czermak's, and I therefore abstain from describing it. The latter, with recent modifications, can now be obtained at Mr. Hawkesley's, 4 Blenheim Street, Bond Street, London, to whom, I should say, I have been greatly indebted for the care and thought he gave to successive and very numerous modifications of my instruments (far more numerous than I care to describe). A drawing of Czermak's apparatus will be found in the 'Berichte der K. Sächs. Gesellschaft der Wissenschaften zu Leipzig,' 1867, p. 212.

For injections, I used a five-drachm ebonite syringe, whose stem was boldly graduated to drachms. The canula (to be inserted into the vein) was screwed into a light stopcock. This was filled with water, which, so long as the cock was closed, did not run out for want of a vent-hole. When it was thrust in the vein and the vein was tied round it, I held the syringe full of blood near the open end of the stopcock, drove out all air by allowing a few drops of blood to fall into its mouth, then pushed its nozzle firmly in, opened the cock and began to inject, steadily and slowly, at the rate of about one drachm in twenty seconds. When the syringe was emptied, I turned the stopcock, withdrew it, rapidly filled it, emptied it and again filled it with warm water, and returning to the canula with the same precautions as before, I threw in about $\frac{1}{2}$ drachm, to wash the blood out of the canula and adjacent vein. I do not think I lost more than three (or perhaps four) rabbits by injecting air, although the removals and replacements of the syringe were very numerous, often ten times in a single operation of the *in kind*.

My apparatus consisted of a zinc warm-water bath, represented on the left of the diagram (p. 408); the vessels drawn to the right of it fitted into holes in its lid, as indicated by the letters. A is the basin to catch the supply blood;



it was whipped up by the whisk F; then poured into C, which consists of a short funnel with muslin below, resting in the top of a glass measure; when the blood had strained through, the funnel and muslin were set on the top of D, to get them out of the way and, at the same time, to keep them warm for future use; B is the thermometer; E is a spill-case full of water to contain the syringe. In addition to these, I required a large slop-pail, a jug of hot, and another of cold water.

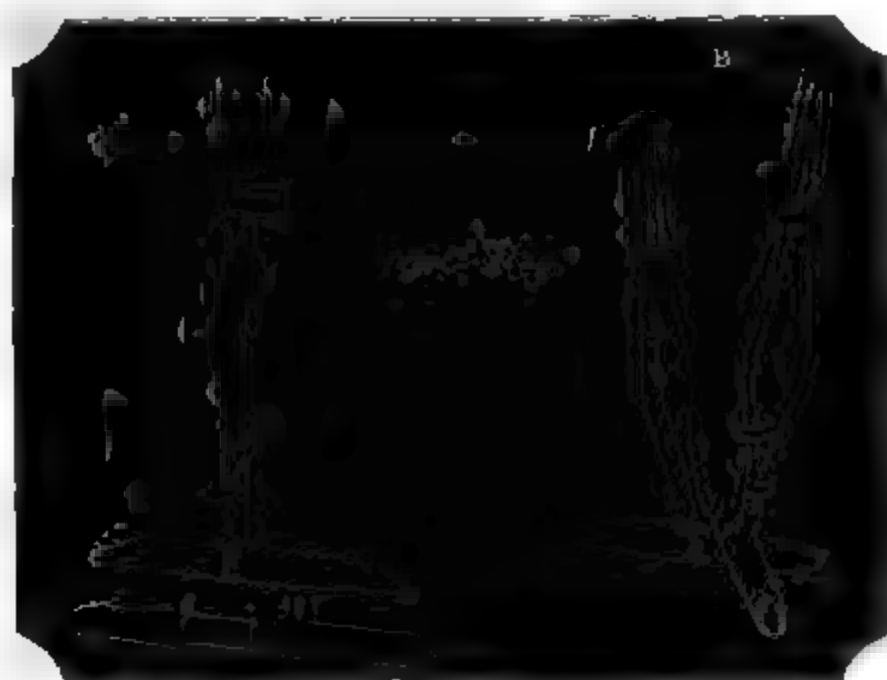


The sketch shows my latest outfit of basins and warm water for injecting. It was not perfected until I had nearly finished the experiments. Scrupulous cleanliness is requisite, and great orderliness; for the hazard lies, not in the performance of one difficult operation, but in making a mistake in some one of a great many easy operations. The course of an operation was as follows:—(1) secure the animal, (2) remove fur from neck, (3) anæsthetics, (4) expose jugular, (5) cut a slit in it and let the animal bleed as much as he can easily bear, about six drachms, (6) stop the flow with gentle pressure by spring forceps; the animal was then left for a minute while (7) Dr. Murie and Mr. Fraser divided the throat of the supply-rabbit, I catching the blood in a warmed basin and whipping it up, to defibrinize it, as it fell. I continued doing this while Dr. Murie was (8) inserting the canula; and when he was nearly ready he called to me, and I (9) filtered the blood, noting its amount, as a guide to what I had to dispose of, (10) drew up a syringe full, (11) injected a convenient number of drachms or half drachms, indicated by the graduations on the syringe-handle, (12) returned the overplus to the glass of supply-blood, (13) cleansed syringe and injected water, (14) let the rabbit bleed three or four drachms,—and then recommenced the series. I have not re-inserted in this description before (11) and (13) what I previously described about turning the stopcock &c.; nor have I spoken of the continual jotting down of notes in my case-book.

At the end of all, the vein was tied. It was, no doubt, the surest plan to avoid future hemorrhage, especially as the blood was defibrinized; but the rabbits were apt to suffer from phlebitis, and I lost some thereby.

Owing to the extreme rapidity and stiffness of the coagulation of rabbit's blood, it is quite easy to estimate the quantity that may have been spilt on the operating-table. It has simply to be sponged into a measuring-glass.

Cross-circulation would be a very easy operation in animals whose carotids were even a trifle larger than those of silver-grey rabbits; but it is difficult with these, because the smallest canula which can be used with propriety can only just be forced into the largest of them. It is no use operating with small canulæ; in every case, a layer of fibrine is sure to line the tube; if the bore is small this layer chokes it, while a layer of equal thickness in a larger tube leaves a free central passage. I found canulæ $\frac{1}{20}$ inch in diameter of bore were worthless; those I used were $\frac{1}{10}$ inch. If I were to operate again, I should not use silver-grey rabbits, on account of their smallness, but "Belgian hare" rabbits. When the canulæ are brought home together, the wire hooks, shown in the sketch, secure them; but I also slipped an India-rubber band over the tips of their handles. The cut ends of the artery were held open and stretched out by a pair of delicate curved forceps (a suggestion due to Dr. Murie), and the



canula was pressed in (the shape of its mouth was the result of many trials and modifications), and a ligature was put on. In the diagram, A represents one pair of canulæ, both opened and closed. B shows their position at the time of crossed circulation. It will be observed that each artery requires four pieces of apparatus, viz. two spring forceps to stop the blood, and two canulæ. Thus, when the throats were brought close together, to connect the arteries cross-wise, there were no less than eight

separate pieces at work in a deep hollow, close together, and attached to delicate arteries, none of which could be permitted to twist or interfere with each other. I append a reduced sketch of one of the two frameworks over which, as previously described, I suspended these instruments, with attached counterpoises, and so avoided all confusion. Both pair of canulæ and two pair of forceps are here represented; they might be so arranged; but it is better to divide the instruments, equally, between the two frames.



For removing clogs from the canulæ, I tried a great many plans, none with as much success as I could wish. I have, however, been able to extract clots from the artery itself, a good quarter of an inch beyond the canulæ, with a wire whose end had been cut with a file into a delicate solid corkscrew.

I washed out the canulæ, before reconnecting, with a thin stream of water sent through the quill of a small bird, which I had fastened, by help of a short India-rubber tube, to my syringe.

The wounds require careful dressing, just like those of a man. The rabbits bear the operations wonderfully well, and appear to suffer little or no pain when the influence of the anæsthetics happens to have left them temporarily sensible. They are often quite frisky when released, and sometimes look as though nothing whatever unusual had happened to them, all through the time of their recovery.

11. "Contributions to the History of Orcin.—No. I. Nitro-substitution Compounds of the Orcins"*. By JOHN STENHOUSE, LL.D., F.R.S., &c. Received March 1, 1871.

The action of nitric acid upon orcin has been studied by several chemists, but with comparatively negative results. Schunck † in this manner obtained a red resinous substance, which by further treatment with the acid was oxidized to oxalic acid; and in 1864 De Luynes ‡ found that orcin dissolved in cooled fuming nitric acid without evolution of nitrous fumes, and that the addition of water precipitated a red colouring-matter; the long-continued action of the vapour of fuming nitric acid on powdered orcin likewise produced a red dye apparently identical with the above. These, however, were resinous uncrystallizable substances.

Although under ordinary circumstances only resinous products are obtained by treating orcin with nitric acid, yet, when colourless orcin in fine

* A Preliminary Notice with this title was published in the 'Chemical News,' August 26, 1870.

† Ann. Chem. Pharm. vol. liv. p. 270.

‡ Ibid. vol. cxxx. p. 34.

powder is gradually added to strong nitric acid cooled by a freezing-mixture, it dissolves with a pale brown coloration, but without the slightest evolution of nitrous fumes. If this solution be now slowly dropped into concentrated sulphuric acid cooled to -10°C. , the mixture becomes yellow and pasty, from the formation of nitro-orcin, which is but slightly soluble in sulphuric acid. When this is poured into a considerable quantity of cold water, the nitro-body separates as a bright yellow crystalline powder, quite free from any admixture of resin.

After numerous experiments the following was found to be the most advantageous process for the preparation of nitro-orcin. 6 grms. of colourless orcin were dissolved in 6 cub. centims. of boiling water, and when the solution had cooled to about 50°C. , it was added in small portions at a time, and with constant stirring, to 40 cub. centims. of nitric acid, sp. gr. 1.45, which was maintained at a temperature of about -10°C. by immersion in a good freezing-mixture. The solution, which was of a very pale brown colour, was now added, in a similar manner, to 120 cub. centims. of concentrated sulphuric acid, also maintained at -10°C. The pasty mass, after being allowed to stand for fifteen or twenty minutes in the freezing-mixture, was poured into a beaker containing 300 cub. centims. water and 400 grms. ice; the crude nitro-compound was then precipitated as a yellow or orange-coloured granular powder. The orcin employed in the preparation of this nitro-compound was colourless, having been purified by distillation *in vacuo*. The yield of crude nitro-orcin amounted to 150 per cent. of the weight of the orcin.

The crude nitro-orcin was collected, washed with a little cold water, and purified by one or two crystallizations from boiling water (40 parts). It was thus obtained in large yellow needles, which are readily soluble in hot water and but slightly in the cold; the addition of a strong acid precipitates almost the whole of the nitro-orcin from its cold aqueous solution. It is soluble in alcohol, very soluble in hot benzol, and crystallizes out in great part on cooling; it is less soluble in ether, and but moderately so in bisulphide of carbon. It dyes the skin yellow, like picric acid, but is tasteless. It volatilizes slightly at 100°C. , melts at 162°C. , and decomposes with slight explosion immediately afterwards. When heated with concentrated sulphuric acid it dissolves, forming a deep yellow solution, which deposits crystals on cooling, and is immediately precipitated by water. It dissolves in hot strong nitric acid with evolution of nitrous fumes and formation of oxalic acid. Like picric acid, when treated with calcium hypochlorite it yields chloropicrin at the ordinary temperature. Its aqueous solutions are coloured dark brown by ferric chloride, and completely precipitated by lead subacetate.

The analysis of the substance dried at 100°C. was made, with the following results:—

I. .335 gm. substance gave .400 gm. carbonic anhydride and .062 gm. water.

II. .306 grm. substance gave .366 grm. carbonic anhydride and .060 grm. water.

III. .525 grm. substance gave .629 grm. carbonic anhydride and .096 grm. water.

Theory.	I.	II.	III.	Mean.
C ₇ = 84= 32.43	32.58	32.63	32.68	32.63
H ₃ = 5= 1.93	2.06	2.18	2.03	2.09
N ₃ = 42= 16.22
O ₈ = 128= 49.42
<hr/> 259 100.00				

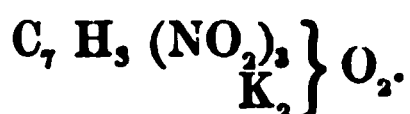
These results correspond to the formula C₇ H₃ (NO₂)₃ O₈, that of trinitro-orcin. It is a powerful acid, much resembling picric acid, but distinguished from the latter by the greater solubility of its salts. I propose, therefore, to call this new substance *trinitro-orcinic acid*.

*Potassium trinitro-orcinat*e.—This was readily prepared from trinitro-orcinic acid by dissolving it in a warm and rather concentrated solution of potassium carbonate. On cooling it solidified to a crystalline mass of fine needles of a deep orange colour, which after the removal of the mother-liquors by the vacuum filter, or by pressure, was purified by crystallization from hot water, in which it was very soluble. The salt dried at 100° was submitted to analysis.

I. .300 grm. substance gave .155 grm. potassium sulphate.

Theory.	I.
C ₇ = 84
H ₃ = 3
K ₂ = 78.2=23.38	23.19
N ₃ = 42 .	..
O ₈ = 128
<hr/> 335.2	

The result obtained agrees with formula



*Sodium trinitro-orcinat*e.—This was obtained by adding trinitro-orcinic acid to a strong solution of sodium hydrate or carbonate until nearly neutralized, and purifying by recrystallization. It forms orange-coloured microscopic needles, strongly resembling the potassium salt.

*Ammonium trinitro-orcinat*e was prepared by adding trinitro-orcinic acid to moderately strong ammonia, in quantity insufficient to neutralize it, boiling for a short time, and then setting aside to crystallize. It forms deep-yellow silky needles, which are very soluble in water, but much less so in alcohol.

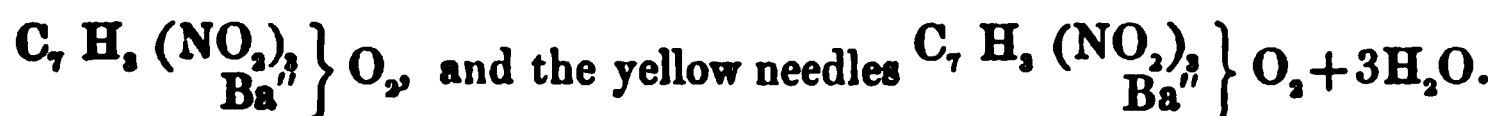
Barium trinitro-orcinat.—Trinitro-orcinic acid was dissolved in 400 or 500 parts of boiling water and an excess of barium carbonate added, the pale yellow solution filtered from the excess of carbonate and set aside. On cooling, the solution formed a semisolid crystalline pap, consisting of fine bright yellow needles of the barium salt. The air-dried salt lost 10·7 per cent. at 100°, and became of an orange-red colour, but regained its original colour on exposure to moist air. A barium determination was made of the salt dried at 100°.

I. ·235 grm. substance gave ·139 grm. barium sulphate.

II. ·328 grm. substance gave ·194 grm. barium sulphate.

	Theory.	I.	II.	Mean.
C ₇ = 84
H ₃ = 3
Ba'' = 137 = 34·77		34·78	34·79	34·78
N ₃ = 42
O ₆ = 128
	<hr/> 394			

The orange-coloured anhydrous salt has the composition



Calcium trinitro-orcinat, prepared by neutralizing the acid with calcic carbonate, is very soluble in hot water, and crystallizes out on cooling in interlaced yellow needles, which are rather soluble in cold water. It is but slightly soluble in hot alcohol; and the solution forms a gelatinous mass on cooling.

Magnesium trinitro-orcinat is very soluble in cold water, but less so in spirit. It crystallizes in minute orange needles.

Lead trinitro-orcinat.—This salt was prepared by dissolving the pure trinitro-orcinic acid in 1000 parts of water strongly acidulated with acetic acid, and adding a solution of lead acetate likewise strongly acidulated, when the lead trinitro-orcinat came out as a bright yellow crystalline precipitate, consisting of tufts of microscopic needles, which are very tough and difficult to reduce to powder. It is almost insoluble in cold water, slightly soluble in hot. It is soluble in hot acetic acid, from which it crystallizes unaltered. It is insoluble in alcohol. If alcoholic solutions of trinitro-orcinic acid and lead acetate be mixed, the lead-salt is obtained as a yellow amorphous precipitate.

Dried at 100° it gave the following results:—

I. ·319 grm. substance gave ·207 grm. plumbic sulphate.

II. ·536 grm. substance gave ·348 grm. plumbic sulphate.

III. ·339 grm. substance gave ·221 grm. plumbic sulphate.

	Theory.	I.	II.	III.	Mean.
C ₇ = 84
H ₃ = 3
Pb = 207 = 44·62		44·34	44·36	44·54	44·41
N ₃ = 42
O ₈ = 128
	<hr/> 464				

Its composition is therefore represented by the formula



*Copper trinitro-orcinat*e, formed by dissolving cupric carbonate in a strong aqueous solution of trinitro-orcinic acid, was with difficulty obtained in the crystalline state in the form of small reddish-brown needles, which are very soluble in water and alcohol, but are precipitated from their solution in the latter menstruum by ether.

*Zinc trinitro-orcinat*e, formed from zinc oxide in a similar manner to the copper salt, is likewise very soluble in water and alcohol, and crystallizes in tufts of yellow needles.

*Silver trinitro-orcinat*e.—Trinitro-orcinic acid was dissolved in fifty times its weight of boiling water, an excess of silver oxide added, and, after boiling a few minutes, filtered from the undissolved silver oxide. On cooling, the whole solidified to an orange-red gelatinous mass of the silver compound, which exhibited no signs of crystallization. It is moderately soluble in hot water; and its solution gelatinizes on cooling. When boiled for any length of time its solution slowly decomposes. Dried at 100° it gave the following results:—

I. ·458 grm. substance gave ·277 grm. argentic chloride.

II. ·398 grm. substance gave ·241 grm. argentic chloride.

	Theory.	I.	II.	Mean.
C ₇ = 84
H ₃ = 3
Ag ₂ = 216 = 45·66		45·41	45·57	45·49
N ₃ = 42
O ₈ = 128
	<hr/> 473			

This corresponds to the formula



*Ethylic trinitro-orcinat*e.—Dry and finely powdered silver trinitro-orcinat was digested with ten times its weight of ethylic iodide until the silver salt was completely decomposed, as shown by the pale yellow colour of the silver iodide formed, the excess of ethylic iodide distilled off, and the ethyl

trinitro-orcinat extracted from the residue by boiling alcohol, from which it crystallized in bright yellow prismatic needles. One or two recrystallizations rendered it quite pure. It melts at $61^{\circ}5$ C., and is very soluble in hot alcohol, from which, if the solution be concentrated, it is deposited as an oil that solidifies on cooling.

The substance dried *in vacuo* was submitted to analysis.

I. .189 grm. substance gave .290 grm. carbonic anhydride and .078 grm. water.

	Theory.	I.
$C_{11} = 132 =$	41.91	41.85
$H_{13} = 13 =$	4.13	4.58
$N_3 = 42 =$	13.33	..
$O_4 = 128 =$	40.63	..
	<hr/>	
	315	100.00

It may therefore be represented by the formula



Methyl trinitro-orcinat was prepared in a manner precisely similar to that above described, substituting methylic for ethylic iodide. Like the ethyl compound, it forms bright yellow crystals, which, however, have a somewhat higher melting-point, viz. $69^{\circ}5$ C.

Trinitro-resorcinic Acid.

As it seemed important to ascertain if the homologues of orcin yielded compounds similar to trinitro-orcin, I submitted resorcin to the action of nitric and sulphuric acids in a manner precisely similar to that above described for the preparation of trinitro-orcin.

The resorcin employed was prepared from Galbanum by the excellent method given by its discoverers, Hlasiwitz and Barth*, substituting, however, sodium hydrate for potassium hydrate. After purification it was treated with nitric and sulphuric acids, employing the proportions and methods detailed in the preparation of trinitro-orcin. As might have been expected, the yield of crude substance was larger, being 180 per cent. of the resorcin employed. It was collected, washed with cold water, and purified by one or two recrystallizations from boiling water (30 parts). It is of a paler colour than the corresponding orcin compound, and crystallizes in leafy plates. It melts at a higher temperature, viz. $175^{\circ}5$ C., and is more soluble than trinitro-orcin; 156 parts of water dissolve one of trinitro-resorcin at 14° C., but the presence of even a small proportion of one of the stronger acids renders it almost insoluble. Dried at 100° it gave the following results:—

* Ann. Chem. Pharm. vol. cxxx. p. 354.

I. .384 grm. substance gave .412 grm. carbonic anhydride and .043 grm. water.

II. .418 grm. substance gave .451 grm. carbonic anhydride and .051 grm. water.

	Theory.	I.	II.	Mean.
C ₆ = 72	= 29.39	29.27	29.43	29.35
H ₇ = 3	= 1.23	1.30	1.36	1.33
N ₃ = 42	= 17.14
O ₉ = 128	= 52.24
	<hr/> 245			
	100.00			

This corresponds to the formula C₆ H₇ (NO₂)₃ O₉, that of *trinitro-resorcin*.

*Barium trinitro-resorcin*ate was prepared by dissolving trinitro-resorcin in 100 parts boiling water and adding an excess of baric carbonate. The filtered solution, on cooling, deposited the barium compound in minute rhomboidal plates of a pale yellow colour. It is much more soluble than the corresponding trinitro-orcin compound. The crystals contain three equivalents of water of crystallization, which they do not lose at 100° C.; but when gently heated on platinum foil they assume an orange-red colour from the loss of water of crystallization, and at a higher temperature explode with extreme violence, perforating the foil. Both trinitro-orcinic and resorcinic acids and their salts explode with far greater violence than picric acid and its compounds.

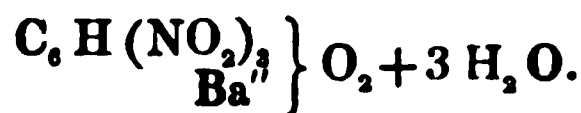
A barium determination of the nitro-resorcin, dried at 100°, yielded the following results:—

I. .409 grm. substance gave .220 grm. baric sulphate.

II. .444 grm. substance gave .239 grm. baric sulphate.

	Theory.	I.	II.	Mean.
C ₆ = 72
H ₇ = 7
Ba'' = 137.2	= 31.59	31.63	31.66	31.64
N ₃ = 42
O ₁₁ = 176
	<hr/> 434.2			

The formula is therefore



*Lead trinitro-resorcin*ate.—The addition of a solution of acetate of lead to an aqueous solution of trinitro-resorcinic acid gave a yellow gelatinous precipitate of lead trinitro-resorcin. It was found best, however, to pre-

pare it by boiling a solution of one part of trinitro-resorcinic acid in 200 of water, with a slight excess of lead carbonate, when the lead compound was deposited on cooling in the form of deep yellow needles. It is but slightly soluble in water, very soluble in acetic acid, and precipitated therefrom by the addition of alcohol. Subacetate of lead completely precipitates solutions of trinitro-resorcinic acid.

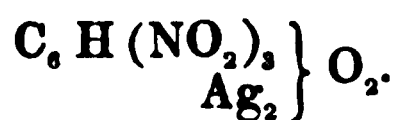
Silver trinitro-resorcinate.—This was prepared in a manner similar to the trinitro-orcinatate. A solution of one part of the pure acid in 75 of water was boiled for a short time with a slight excess of oxide of silver and filtered. On cooling, the silver-salt was deposited in long fine needles of a yellowish-brown colour. They crystallize readily from boiling water, in which they are much more soluble than the corresponding trinitro-orcinatate. Dried at 100° it gave the following results:—

I. .456 grm. substance gave .284 grm. chloride of silver.

II. .416 grm. substance gave .260 grm. chloride of silver.

	Theory.	I.	II.	Mean.
C ₆ = 72
H = 1
Ag ₂ = 216 = 47.07		46.87	47.04	46.96
N ₃ = 42
O ₉ = 128
	<hr/> 459			

The analyses correspond to the formula



Trinitro-beta-orcinic Acid.

Beta-orcin, C₆ H₁₀ O₂, when treated with nitric and sulphuric acids as above described, gives a yellow substance, which appears to be the corresponding nitro-compound of beta-orcin; but from the small amount of material at my disposal, I am at present unable to accurately determine its properties.

From some experiments I have made, the action of reducing agents, nitrous acid, &c. upon these nitro-compounds promises very interesting results. I am at present investigating this subject. My thanks are due to Mr. Charles E. Groves for the efficient assistance he has rendered in conducting the above investigation.

The Society adjourned over the Easter Recess to Thursday, April 20th.

Presents received, March 2, 1871.

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Dr. WILLIAM HUGGINS, Vice-President, in the Chair.

I. "Note on the circumstances of the Transits of Venus over the Sun's Disk in the years 2004 and 2012." By J. R. HIND, F.R.S. Received March 1, 1871.

The following are the resulting elements of the transit in 2004 :—

Greenwich mean time of conjunction in right ascension 2004, June 7^d 20^h 51^m 28^s.8.

Right ascension of Sun and Venus	76° 50' 28.6
Declination of Sun	+22 53 20.4
„ Venus ..	+22 42 52.3
Horary motion in R.A. Sun	2 35.07
„ Venus ..	—1 37.40
Horary motion in declination. Sun	+0 13.00
„ „ Venus ..	—0 43.83
Semidiameter of Sun	15 45.74
„ Venus ..	28.75
Horizontal parallax Sun	8.78
„ Venus ..	30.85
Log. distance of Venus from the Earth ..	9.46069
Equation of time .. 1 ^m 15 ^s .6 (additive to mean time).	

Hence, for the centre of the earth,—

		d	h	m	s				
First external contact ...	June	7	17	3	43	at	115°0	from N. towards E.	
„ internal	„		17	22	35	at	118·0	„	} For the direct image.
Second internal	„		23	5	40	at	214·6	„	
„ external	„		23	24	32	at	217·5	„	

And l being the geocentric latitude, ρ the radius of the earth at any place,

and λ the longitude from Greenwich + E., — W., the reductions for parallax will be obtained from

	d	h	m	s	
1st ext. cont.... June 7	17	3	43	+	$[2.2198]\rho \cdot \sin l - [2.5932] \cdot \rho \cdot \cos l \cdot \cos(\lambda + 176^\circ 32')$
1st int. cont. „	17	22	35	+	$[2.2571]\rho \cdot \sin l - [2.5765] \cdot \rho \cdot \cos l \cdot \cos(\lambda + 182^\circ 38')$
2nd int. cont. „	23	5	40	—	$[2.5090]\rho \cdot \sin l + [2.4353] \cdot \rho \cdot \cos l \cdot \cos(\lambda + 47^\circ 17')$
2nd ext. cont. „	23	24	32	—	$[2.4928]\rho \cdot \sin l + [2.4631] \cdot \rho \cdot \cos l \cdot \cos(\lambda + 54^\circ 35')$

For the Royal Observatory, Greenwich, I find:—

	d	h	m	s	
First external contact, June 7	17	9	56		} Mean times at Greenwich.
„ internal „ „	17	28	51		
Second internal „ „	23	3	24		
„ external „ „	23	22	15		

Therefore the entire transit will be visible at Greenwich.

Similarly the elements of the transit of 2012 are found to be:—

Greenwich mean time of conjunction in right ascension 2012, June 5^d 13^h 4^m 44^s.3.

Right ascension of Sun and Venus	74° 31' 11.9
Declination of Sun	+22 40 24.1
„ Venus	+22 50 3.0
Horary motion in R.A. Sun	2 34.67
„ „ Venus	—1 37.70
Horary motion in declination. Sun	+0 15.23
„ „ Venus	—0 45.37
Semidiameter of Sun	15 46.01
„ Venus	28.77
Horizontal parallax Sun	8.76
„ „ Venus	30.86
Log. distance of Venus from the Earth..	9.46042
Equation of time .. 1 ^m 19 ^s .8 (additive to mean time).	

Hence, for the centre of the earth,—

	d	h	m	s		
First external contact ... June 5	10	22	11	at	40.3	from N. towards E.
„ internal „ „	10	39	56	at	37.8	„
Second internal „ „	16	42	6	at	293.1	„
„ external „ „	17	0	0	at	290.5	„

} For the
direct
image.

And, with the same notation as before, I find for the reductions for parallax,—

	d	h	m	s	
1st ext. cont. ... June 5	10	22	11	+	$[2.4536]\rho \cdot \sin l - [2.4582] \cdot \rho \cdot \cos l \cdot \cos(\lambda + 41^\circ 28')$
1st int. cont. „	10	39	56	+	$[2.4838]\rho \cdot \sin l - [2.4558] \cdot \rho \cdot \cos l \cdot \cos(\lambda + 43^\circ 52')$
2nd int. cont. „	16	42	6	—	$[2.1301]\rho \cdot \sin l + [2.5968] \cdot \rho \cdot \cos l \cdot \cos(\lambda - 10^\circ 57')$
2nd ext. cont. „	17	0	0	—	$[2.1158]\rho \cdot \sin l + [2.5825] \cdot \rho \cdot \cos l \cdot \cos(\lambda - 6^\circ 28')$

At Greenwich the egress only will be visible.

				^h	^m	^s	
Last internal contact,	June 5,	at	16	44	23	} Mean times at	
„ external	„	„	17	2	15		Greenwich.

The sun will rise at 15^h 46^m.

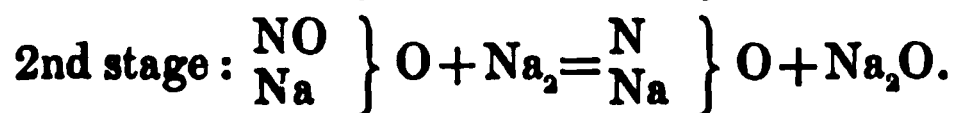
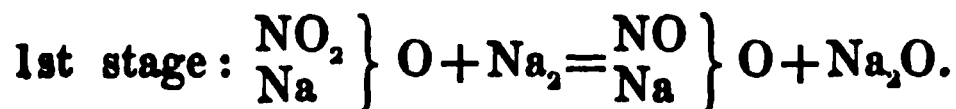
II. "On the Existence and Formation of Salts of Nitrous Oxide."

By EDWARD DIVERS, M.D. Communicated by Professor W. ODLING, M.B., F.R.S. Received March 2, 1871.

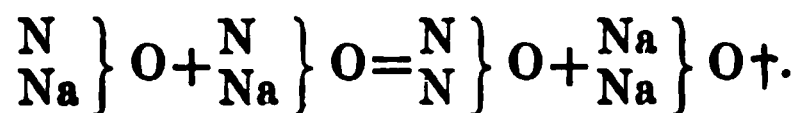
1. Metallic sodium thrown on a solution of an alkali nitrate was found by Schönbein* to reduce it to nitrite. He contented himself, however, with merely detecting the nitrite by the iodide and starch test. By using the sodium in the form of amalgam the complete reduction of the nitrate to nitrite can be readily effected, and silver nitrite freely precipitated from the solution by first neutralizing it with an acid and then adding silver nitrate.

2. But so soon as nitrite is thus formed by the sodium, it itself begins to suffer reduction, as well as the remaining nitrate, by the action of more sodium. This reduction of the nitrite is rendered evident by the effervescence which attends it, the gas given off consisting of pure nitrous oxide. If excess of sodium amalgam be gradually added to the nitrate solution, and its action moderated by keeping the vessel containing the mixture in a stream of cold water, the effervescence only becomes very lively when the sodium added has nearly reached the proportion of two atoms to one of the nitrate used. When four atoms of sodium have been oxidized by the solution, the further addition of it is without effect; no more effervescence takes place, and the sodium remains unchanged in the mercury.

3. The very alkaline liquid which is left by the reaction contains a new salt, though in relatively small quantity—the salt of nitrous oxide. The action of sodium on sodium nitrate may therefore be thus formulated:—



As regards the escape of nitrous oxide during the reduction, this is explained by the reaction on each other of two molecules of the new salt, under the influence of the heat produced by the oxidation of the sodium, thus:—



* Erdmann's Journ. für prakt. Chemie, vol. lxxxiv. (1861) p. 202.

† When ammonium nitrate is employed instead of sodium or potassium nitrate, the action of the sodium is the same; and it is here interesting to point out that ammonium nitrate is an exception to the conclusion at which Gay-Lussac and Thénard arrived (Journ. de Physique, vol. lxi. 1809, p. 463), after they had tried the carbonate, chlo-

4. After neutralizing the alkaline liquid by acetic acid, it gives a yellow, pulverulent precipitate with silver nitrate. This precipitate, when thoroughly washed from its saline mother liquor, is almost as insoluble in water as silver chloride; for hydrochloric acid gives no immediate opalescence with water filtered through it. It is quite stable below 100° C., or a little lower than this, and it may be washed with hot water without change. It is also unaffected by light, or by exposure to a pure atmosphere, even when in contact with paper. It is but very sparingly soluble in acetic acid; so that this acid may be added in excess to the original alkaline liquid without removing its property of being precipitated by silver salts. It is soluble in ammonia and ammonium carbonate, from solution in either of which it can be again thrown down by acetic acid, or by cautious neutralization of the ammonia by dilute nitric or sulphuric acid, or by the volatilization of the ammonia. It is soluble in either dilute nitric or sulphuric acid, and without immediate decomposition; and it can be reprecipitated from its solution in either of these acids by the cautious addition of ammonia or ammonium carbonate, or by the free addition of sodium carbonate or sodium hydrate, in either of which it is insoluble. It is immediately oxidized by concentrated nitric acid, copious red fumes being produced. Moderately diluted nitric, sulphuric, or hydrochloric acid decomposes it, with the evolution of nitrogen, and the production of apparently both nitrous and nitric acids in the solution. It is also decomposed by soluble chlorides and by hydrosulphuric acid. When precipitated from the original liquid, it sometimes becomes dark-coloured; but this change is due to the formation of a black matter derived from the sodium or naphtha and the silver acetate. After washing it, dissolving it in very dilute nitric acid, and filtering the solution, it may be reobtained by the cautious addition of ammonia, or by the addition of ammonia to alkaline reaction and then a little acetic acid, in a condition in which it is no longer liable to discoloration. It is decomposed by a moderate heat into nitric oxide, metallic silver, and a little silver nitrate—in this respect resembling silver nitrite. My experiments on silver nitrite, about being published, show that nitric oxide may serve as a carrier of atmospheric oxygen to silver nitrite; and it is therefore most probable that in this case also some of the nitric oxide liberated serves to carry a little oxygen to the still undecomposed new silver-salt. During its decomposition by heat it does not fuse or exhibit any other change except that from a bright yellow to a silver-white colour. After a red heat nothing remains but pure silver.

5. The following determinations of the amount of silver in the salt have been made:—

I. .3317 grm., dried at a gentle heat, was suspended in water and digested

ride, phosphate, and sulphate, that any ammonium salt would give ammonium amalgam with sodium amalgam. Ammonium nitrate yields no ammonium amalgam with sodium amalgam; more than this, the presence of a nitrate prevents the formation of this body by other salts of ammonium.

with hydrochloric acid. The silver chloride weighed $\cdot 3386$ grm. = $\cdot 2549$ grm. silver.

II. $\cdot 5703$ grm. of some of the salt which had been dissolved in ammonia, filtered, and precipitated by acetic acid, was dissolved in dilute nitric acid and treated with hydrochloric acid. It yielded $\cdot 5858$ grm. silver chloride = $\cdot 4410$ grm. silver.

III. $\cdot 9462$ grm. of some of the salt which had been dissolved in nitric acid, filtered, and reprecipitated by ammonia, was dissolved in very dilute nitric acid and precipitated by hydrochloric acid. It gave $\cdot 9722$ grm. chloride = $\cdot 7318$ grm. silver.

IV. $\cdot 6111$ grm. of the same preparation as the last was heated at first moderately and then to redness. It left $\cdot 4737$ grm. silver.

V. $\cdot 3685$ grm. kept for sixty hours at a temperature varying from 100° to 175° , left a residue weighing $\cdot 2921$ grm., of which $\cdot 2775$ grm. was metallic silver and the rest silver nitrate = $\cdot 0093$ grm. silver.

VI. $\cdot 5937$ grm. heated moderately till red fumes ceased to appear yielded $\cdot 4274$ grm. silver and $\cdot 0557$ grm. silver nitrate = $\cdot 0354$ grm. silver.

The percentage numbers for the silver found and that required by the formula NOAg are :

Calc.	I.	II.	III.	IV.	V.	VI.
78.26	76.85	77.33	77.34	77.48	77.83	77.95

so that there can be no doubt as to the composition of the salt, although the silver comes out about one per cent. too low. But in several of the samples traces of brownish or black matters were detected on solution ; and in the last preparation analyzed, the presence of a little moisture was detected, an attempt to expel which in some of the unused salt by a stronger heat caused partial decomposition.

6. If the product of the ultimate action of sodium on the nitrate be treated with acetic acid, as directed, until it is neutral to test-paper, it gives no precipitate with any metallic solution except that of silver, so far at least as trial has yet been made. But the precipitation of the silver-salt leaves the solution of an acid reaction to litmus ; and even if the solution before precipitation be rendered a little alkaline to litmus, it will, after precipitation, generally react slightly acid. The reason of this is clearly that the sodium salt has a markedly alkaline reaction ; and this is further shown to be the case by adding some of the washed silver-salt to a solution of potassium or sodium chloride, which it at once renders alkaline in reaction to litmus. To obtain a solution of the salt free from acid or caustic alkali, the original alkaline liquid is to be treated with acetic acid in successive quantities until it just ceases to yield a brown precipitate with silver nitrate. Such a solution will yield the yellow silver-salt with silver nitrate as before, and also precipitates with the salts of most of the common metals. Instead of acetic acid dilute nitric acid may be used in this method for thus neutralizing the caustic soda, with the same result as to

the alkalinity of the solution to test-paper, and its capability of being precipitated by many metallic salts.

7. The following reactions having as yet been only observed with a solution thus obtained, it is possible that some of them may not be due simply to the new acid:—

Barium chloride gives no precipitate.

Lead acetate gives a cream-white flocculent precipitate, which generally, on standing, changes to a very dense, full yellow precipitate. This precipitate is unchanged by boiling in water or in its mother liquor, is soluble in acetic and other acids, slowly, if at all, affected by ammonia or sodium carbonate, and instantly decomposed by caustic soda.

Mercuric chloride gives a cream-white flocculent precipitate.

Mercurous nitrate gives a blackish-grey precipitate, not improbably a mixture of mercury and the mercuric salt.

Cupric sulphate, a yellowish olive-green flocculent precipitate, soluble in acids and ammonia, insoluble in caustic soda, and unaffected by boiling in water or its mother liquor. The colour of this precipitate closely resembles the colour of a copper-salt mixed with a nitrite.

Zinc chloride gives a white precipitate.

Manganese chloride gives a whitish precipitate.

Nickel chloride gives a greenish, almost white precipitate.

Cobalt nitrate gives no precipitate(?).

Alum gives a white precipitate.

Ferric chloride gives a slight reddish-brown precipitate.

Ferrous sulphate gives a whitish precipitate, instantly darkening to dirty blackish green and eventually reddish brown. On addition of ferric chloride effervescence slowly commences; and with ferrous sulphate the same thing happens, but still more slowly. It is very probable that the iron and aluminium precipitates are simply hydrates; and if so, the action of this salt in these cases closely resembles that of a carbonate.

Silver nitrate behaves as already described.

Sodium chloride gives an alkaline solution with the silver-salt, which is changed to chloride.

Ammonium chloride gives the same result as sodium chloride, but the solution at once evolves ammonia. The existence, therefore, of the ammonium salt of the new acid is problematical.

Potassium permanganate gives the beautiful changes of colour from its own violet, through purple, blue, and chrome-green, to manganate green, and then a brown precipitate. The exhibition of this series of colours is more certain in the presence of a little caustic alkali. In this reaction the new salt resembles a nitrite, but it is much more sensitive than this.

Potassium iodide gives no reaction; it does with nitrite.

Iodine solution is decolorized immediately; so that if a nitrite, not in excess, is added to a solution of the new salt, the addition of starch and potassium iodide produces no coloration.

The solution acidified with acetic or hydrochloric acid still gives no reaction with *potassium iodide*, decolorizes *iodine solutions*, and prevents the action of a nitrite on an iodide.

The acidified solution gives no coloration with *ferrous sulphate*. In contact with strong sulphuric acid, the mixture gives the coloration like a nitrate. As is well known, a nitrite, even without addition of acid, gives an immediate colour with ferrous sulphate.

The acidified solution immediately decolorizes *potassium permanganate*.

The acidified solution does not reduce *potassium dichromate*.

The solution which has been neutralized with nitric acid instead of acetic acid will not do for the iron and iodine tests, as this behaves, so far as it has been tried, as though it contained a nitrite.

The acidified acetic-acid solution, when heated, evolves *nitrous oxide*. Here again, therefore, the new salt is analogous to a carbonate,—



8. When silver nitrite is heated until a greenish-yellow, semifused mass remains, and this is washed out with water, the residue consists of metallic silver and a little bright yellow matter, unaffected by light, soluble in ammonia, and decomposed by boiling in water, as will be found described in my paper on the action of heat on silver nitrite already referred to. From the properties of this yellow substance, and from the manner in which it is formed, it is probable that it is the silver-salt of the new acid; but in consequence of the small quantity of it obtainable, and of the admixture of this with metallic silver, a fuller examination of it has not been attempted. If it be this salt, its formation is analogous to that of silver nitrite by heating silver nitrate.

9. There is also reason for believing that Hess* came across this salt by first treating a solution of barium nitrate which had been deoxidized by heat with a solution of silver sulphate and evaporating the mixture, and then decomposing the crystals thus obtained by the action of water.

10. In his 'Researches on Nitrous Oxide'†, Sir Humphry Davy described some experiments by which he obtained what appeared to him to be a combination of nitrous oxide with potash. He prepared it by exposing a mixture of solid potassium hydrate and potassium sulphite to the prolonged action of nitric oxide, dissolving the resulting product in water, crystallizing out the potassium sulphate formed, and then evaporating to dryness. The mass thus obtained evolved when heated pure nitrous oxide, amounting to about a fourth of its weight.

Since then, however, Pelouze‡ has obtained, by a modification of Davy's method, the alkali nitrosulphates; and it seems to be now universally

* Pogg. Ann. vol. xii. (1828) p. 257.

† Researches, Chemical and Philosophical, chiefly concerning Nitrous Oxide (1800). Res. ii. Div. i. sect. 6, pp. 254–277.

‡ "Sur quelques combinaisons d'un nouvel Acide formé d'Azote, de Soufre et d'Oxygène," Ann. de Chim. et de Phys. vol. lx. (1835) p. 151.

believed that Davy must have in reality obtained a nitrosulphate and not a simple salt of nitrous oxide. I have not yet had time to repeat Davy's experiments myself, but I wish to point out one well-marked and essential difference between the body obtained by Davy and Pelouze's nitrosulphate, which is that, whereas the former body evolved pure nitrous oxide when heated, the latter gives off nitric oxide; and also that, according to Davy's experiments, Pelouze's modification of the former's process would be fatal to its success in forming the salts of nitrous oxide.

11. There is some difficulty in selecting an appropriate name for the new acid. That of hyponitrous acid naturally suggests itself, as being framed according to the usual method of naming a rising series of oxygen acids, and as associating the new acid with the similarly constituted acid of chlorine; but I feel that in naming the nitrous-oxide acid regard ought to be had to the possible existence of salts of nitric oxide, in which several chemists have believed, indeed, and on better evidence, I think, after a perusal of some of their papers, than is generally supposed. Now, if the nitric-oxide acid should be discovered, and if the term "hyponitric" is to be retained for the acid intermediate to the nitrous and nitric acids, the term "hyponitrous" would belong by analogy to the nitric-oxide acid rather than to the new acid.

This difficulty, however, will vanish if the term hyponitric be allowed to fall out of use and that of nitroso-nitric, already adopted by several chemists, be generally substituted for it as the name of the nitrogen-peroxide acid; for then, the term "hyponitrous" being given to the new acid, there remains the compound term "hyponitroso-nitrous" for the nitric-oxide acid, should this acid ever be obtained.

There will thus be the following series of names:—

Hyponitrous acid	HNO
Hyponitroso-nitrous acid	H ₂ N ₂ O ₃
Nitrous acid	HNO ₂
Nitroso-nitric acid	H ₂ N ₂ O ₃
Nitric acid	HNO ₃

If, however, the terms "hyponitrous" and "hyponitric" are to be retained for the second and fourth members of the above series, the term "hydro-nitrosylic" may serve for the new acid, according to its constitution:—

Hydrogen.	Nitrosyl.
H	NO

I am at present engaged in the further study of this interesting acid and its salts, and hope, before very long, to have the honour to make known the obtained results to the Society.

ADDENDUM, April 26, 1871.

When the above paper was presented to the Royal Society I was not

aware that the action of sodium amalgam upon alkali nitrites had been recently investigated by M. Fremy* and M. Maumené†, neither of whom, however, have anticipated me in the discovery of the new class of salts here described. The latter chemist finds that in one set of circumstances this action gives rise to a body allied in composition and properties to oxyammonia, and in another only to ammonia. M. Fremy finds that it produces oxyammonia, nitrogen which escapes, and nitrous oxide which remains in solution.

So far as I have since been able to experiment, I have found that, by proceeding in different ways, a very small and variable amount of oxyammonia, or a substance resembling it, is often obtained along with the sodium hyponitrite, which is always formed in material quantity; and, further, that, under the circumstances which favour the formation of oxyammonia, there are also obtained, together with nitrous oxide, the products of the decomposition of oxyammonia by alkalies—nitrogen and ammonia (Lossen). The presence of oxyammonia in the product of the action of sodium on sodium nitrate affords a more satisfactory explanation than that I have given of the darkening of the silver-salt after precipitation which I have sometimes observed to occur.

M. Fremy states that alkali nitrites do not decompose potassium permanganate; and I now find that my assumption to the contrary is an error, based on an observation of considerable interest, which is that some good commercial nitrite in my possession slowly reduces the permanganate. I have since ascertained that other samples of sodium nitrite have no action on the permanganate, and that the one which does react with it behaves also with copper- and lead-salts in such a way as to render very probable the presence in it of a minute quantity of my new salt. This formation of hyponitrite, by heating sodium nitrate, if it does really take place, is in accordance with Hess's observations of the action of heat on barium nitrate, and with mine on silver nitrite.—E. D.

III. "Research on a New Group of Colloid Bodies containing Mercury, and certain Members of the series of Fatty Ketones."
By J. EMERSON REYNOLDS, Member of the Royal College of Physicians, Edinburgh, Keeper of the Mineral Department, and Analyst to the Royal Dublin Society. Communicated by ROBERT H. SCOTT, F.R.S. Received January 19, 1871.

Introduction.—About ten years ago, when engaged in the examination of some of the constituents of the rectified wood-naphtha of commerce, I observed that moist and freshly precipitated mercuric oxide was dissolved by the naphtha in the presence of potassium hydrate, and that the resulting alkaline solution possessed highly characteristic properties. It was ascer-

* *Comptes Rendus* (1870), vol. lxx. pp. 66 & 1208.

† *Ibid.* p. 149.

tained that the solution of mercuric oxide under the conditions mentioned depended wholly on the presence of acetone in the crude wood-spirit; since pure acetone, when treated in the same way with diluted alkali and mercuric oxide, afforded the same result, while the remaining constituents of wood-naphtha failed to produce the reaction.

Though obliged at the time to rest content with recording* the observations above referred to, I have since been able to resume the inquiry. As the result, I now venture to lay before the Society the following account of some members of a series of ketone compounds containing mercury, and presenting all the characters of strongly marked *colloid* bodies, though chemically intermediate between the two groups of colloids previously made known by the researches of the late Professor Graham.

Acetone, being the most important member of the fatty ketone group, is the body whose compound with mercury I have studied with chief attention, and I may therefore describe in detail the results obtained with it.

1. *Colloid body obtained by uniting Acetone with Mercuric Oxide.*—When solution of mercuric chloride is slowly added to a mixture of acetone with dilute aqueous solution of potassium hydrate, the mercuric oxide first precipitated is dissolved, with the production of a clear colourless liquid. The addition of the mercurial solution can be continued until a white precipitate makes its appearance, the alkali being still in excess†. If the solution be filtered at this point, an apparently opalescent, yellowish-coloured liquid is obtained‡. If one portion of this alkaline solution be boiled for a few minutes, a thick gelatinous mass suddenly separates, and further ebullition is rendered difficult, if not impossible. Another portion of the liquid, when treated with an acid in slight excess, gelatinizes; and if the original solution be moderately strong, the vessel in which the experiment is made may be inverted without risk of spilling its contents. Finally, if some of the mercuric solution be exposed over sulphuric acid *in vacuo*, it leaves on partial evaporation a gelatinous mass, on the surface of which latter crystals of potassium chloride soon make their appearance. When the desiccation is complete, a yellowish resinoid body is obtained, together with a large quantity of very beautiful acicular crystals of the chloride and a certain amount of potassic carbonate.

The solution of mercuric oxide in potassium hydrate in presence of acetone takes place as easily in alcoholic as in aqueous liquids.

Preliminary experiments similar to the foregoing were sufficient to in-

* Journal of the Royal Dublin Society, vol. iv. p. 114.

† The same result can be obtained when mercuric oxide is precipitated from any of its salts, rapidly washed, and then digested with excess of acetone and potassium hydrate. The best mode of operating, however, is that stated in the text.

‡ The different solutions exhibit a slight opalescence, not completely removable by ordinary filtration. This opalescence appears to be due to the very gradual separation, at ordinary temperatures, of traces of the same anhydrous substance which is very rapidly thrown down at a boiling heat. In composition the latter body is identical with the anhydride obtained by other methods and described further on.

dicating that the chief compound produced in the reaction above referred to might be regarded as a colloid body. I therefore took advantage of the late Professor Graham's beautiful dialytic method* for effecting its purification from crystalloids, and have met with complete success.

a. *Preparation of Colloid Liquid.*—As the preparation of a *strong* solution of the pure acetone mercuric compound suitable for dialysis is attended with some difficulty, I may now describe in detail the mode of operating proved by experience to afford the most satisfactory results.

Forty grammes of pure mercuric chloride are to be dissolved in about 500 cub. centims. of hot water and the solution then allowed to cool, even though crystals of the salt separate. Twenty-nine grammes of potassium hydrate are next dissolved in about 300 cub. centims. of water: 15–20 cub. centims. of acetone should now be placed in a capacious glass balloon, and diluted with 250 cub. centims. of water. The reaction is then to be managed as follows:—About 150 cub. centims. of the alkaline solution should be added to the aqueous acetone, and then 250 cub. centims. of the mercuric chloride gradually poured in. Re-solution of the mercuric oxide first thrown down proceeds slowly at the outset, if the mixture be not warmed. After a time the oxide quickly redissolves, if the contents of the balloon are briskly agitated. When the first half of the mercuric solution has been added, the remaining 150 cub. centims. of potassium hydrate are to be cautiously poured in and the residual mercuric chloride then mixed, with the precautions already stated.

The solution prepared in the manner described is usually turbid, but can be easily filtered clear from the small amount of mechanically suspended matter. The filtrate should next be placed on a large hoop dialyzer, covered as usual with carefully prepared parchment-paper, and the vessel floated on a considerable volume of distilled water. After two days' action the diffusate will be found to contain a large quantity of potassium chloride, some potassium hydrate, and but a very small amount of mercury. The process of diffusion is to be continued, the diffusate being replaced by pure water twice each day, until the liquid on which the dialyzer floats no longer affords a cloud when treated with a solution of silver nitrate. The process may then be considered terminated, and the pure colloidal liquid obtained. The contents of the dialyzer can be now removed, and should be free from all odour of acetone. A few drops, when evaporated to dryness on platinum-foil and the residue ignited, should volatilize completely.

The mode of operating just described affords the strongest colloidal liquid that can be conveniently prepared directly in a pure state; but where degree of concentration is of no importance, I find that it is better to dilute the alkaline mercurial solution with its own volume of pure water just before dialyzing.

The properties of the colloidal liquid will be described most conveniently

* "Liquid Diffusion applied to Analysis," *Philosophical Transactions*, 1861.

after the results of the analyses of the anhydrous mercuric compound separable from it in the pure condition shall have been stated.

b. *Analyses of the Anhydrous Compound*.—I took a considerable volume of the very carefully prepared colloid liquid and divided it in two parts. One portion was very cautiously evaporated to dryness, and the resinoid residue very finely powdered and carefully dried. The second portion was precipitated by the addition of dilute acetic acid, and the gelatinous precipitate washed rapidly and completely with the aid of Bunsen's filter-pump, and the residue dried. The desiccation in each case was effected at first *in vacuo* over sulphuric acid, and finally on the water-bath. The compound easily bears a temperature of 100° C.

On analyzing both products, I found that they were practically identical in composition. The residue of the evaporation of the colloid liquid contained, as might be anticipated, a slightly greater proportion of mercury than the precipitate by dilute acid. The circumstances under which the precipitate was produced, however, were such as to give most confidence in the purity of the product; I have therefore employed this latter or similar preparations in many of the determinations, the results of which will be presently stated.

The compound was found to contain carbon, hydrogen, mercury, and oxygen, and to be free from chlorine. As the presence of the volatile metal introduced some difficulty in the determinations, it is necessary to describe the plan of analysis adopted.

The mercury was in some cases obtained in the metallic state by distillation with quicklime; but I much prefer to digest a weighed quantity of the compound in a sealed tube with dilute hydrochloric acid, and, after complete solution has taken place, to break the tube, precipitate the metal from the contents by sulphuretted hydrogen, and weigh the mercury in the usual way as sulphide. The results are very satisfactory.

The presence of a large proportion of mercury in the compound rendered the accurate determination of carbon and hydrogen somewhat difficult. At the outset of this investigation I arranged a very troublesome process of analysis, similar to that employed by Messrs. Frankland and Duppa in their analysis of mercuric ethide and its analogues. In all later combustions I have adopted the very simple and satisfactory plan of placing in the anterior part of the combustion-tube a layer of fifteen centimetres of any metal capable of easily amalgamating with mercury. Gold- and silver-foil answer the purpose well: no doubt tin-foil might be used in the same way. It is scarcely necessary to add that the temperature of the anterior part of the combustion-tube containing the gold or silver was in each instance very carefully regulated.

The following results were obtained:—

I. .997 grm. of substance gave .9037 grm. of Hg S.

II. .8618 grm. of substance gave .3077 grm. of CO₂ and .1158 grm. of H₂ O.

III. .4940 grm. of another preparation gave .4485 grm. of Hg S.

IV. .6640 grm. afforded .2344 grm. of CO₂ and .0870 grm. of H₂ O.

V. 1.2148 grm. of residue of evaporation of colloid liquid gave 9.562 grm. of Hg.

VI. .8156 grm. of another preparation gave .6425 grm. of Hg.

	Calculated.		Experiment.					
			I.	II.	III.	IV.	V.	VI.
Carbon	72	9.43	...	9.73	...	9.62
Hydrogen	12	1.57	...	1.49	...	1.45
Mercury.....	600	78.53	78.10	...	78.26	...	78.71	78.77
Oxygen	80	10.47
	<hr/>	<hr/>						
	764	100.00						

The composition of the body is therefore well represented by the formula



c. *Properties of the Colloid Body, Hydrated and Anhydrous.*—Analogy would lead us to conclude that the colloidal liquid obtained by dialysis is a hydrate of the body represented by the above formula; but since evaporation *in vacuo* is sufficient to remove the water completely, the hydrate can possess but little stability. Properly speaking, this hydrate is no doubt a true liquid, and as such is miscible with other liquids.

The reaction of this hydrate is neutral to test-papers.

When the aceto-mercuric hydrate contains five per cent. of the anhydrous compound, it will, if quite pure, remain liquid for twelve or fourteen days, toward the end of this time becoming gradually less fluid, until the whole "sets" to a firm jelly. The same result may be brought about in a few seconds by the addition to the perfectly neutral liquid of very minute quantities of any of the following substances:—Hydrochloric, acetic, nitric, sulphuric (incompletely), chromic, oxalic, tartaric, or citric acids; by potassium, sodium, ammonium, barium, and calcium hydrates; by calcium chloride, mercuric chloride, sodium acetate, and other neutral salts. Contact with certain insoluble powders, such as calcium carbonate, and even alumina, induces pectization*.

Elevation of temperature quickly determines the gelatination of the liquid. If containing five per cent. of the ketone compound, a very firm jelly is produced on heating to 50° C. In one experiment, a quantity of the liquid was taken and some bright, carefully cleaned copper-gauze introduced. The liquid did not pectize, nor did any trace of mercury deposit on the copper, after standing for a day. The temperature of the whole was then raised to 50° C.; a transparent jelly was at once produced, of such strength that the vessel in which it was contained could

* In accordance with the nomenclature of Prof. Graham, we must call the liquid colloid hydrate the "hydrosol" of the new compound, the gelatinous hydrate the "hydrogel," and the change from the former to the latter "pectization."

be inverted without any risk of loss. This jelly, inclosing the bright copper-gauze, has remained in my possession for eight months without giving the slightest indications of a disposition to change.

Small zoological specimens, when inclosed in the same way in a jelly of the mercuric ketone compound, were found to keep well when carefully cleansed before they were sealed up in the gelatinous envelope.

By evaporation, a liquid containing eight per cent. of the ketone compound was obtained, but it pectized in a few hours. A two per cent. hydrate retained its liquidity for several months. Once a jelly formed in any of these liquids, I have not succeeded completely in reconverting it to the liquid state by very cautious treatment with potassium hydrate, even when aided by diffusion.

The *alcosol* of the mercuric ketone compound was obtained by the method adopted by Prof. Graham in preparing the corresponding silicic alcoholate,—that is to say, by adding to a one per cent. hydrate an equal volume of alcohol, and exposing the mixture over quicklime until most of the water was removed: the *alcosol* remained. This liquid could be boiled without pectizing; but if the ebullition continued for some time, a jelly was suddenly obtained. This insoluble jelly corresponded to that produced on heating the hydrate, or adding to it any of the bodies capable of pectizing it; in the former case alcohol, and in the latter water, being associated or united with the mercuric ketone compound.

It has now been shown that the new body is capable of affording a *hydrosol* and *hydrogel*, an *alcosol* and *alcogel*; it must therefore be regarded as a very strongly marked member of Prof. Graham's group of these colloids, though chemically differing widely from previously described compounds of this class.

When the colloid hydrate was treated with sulphuretted hydrogen, mercuric sulphide was produced. The liquid filtered from the sulphide yielded *acetone* on distillation. Digestion with dilute hydrochloric acid likewise effected the decomposition of the colloid body, mercuric chloride being produced and acetone liberated. Nitric and sulphuric acids, when dilute, did not decompose the compound with the same facility as hydrochloric acid. Treatment of the *hydrosol* with copper, zinc, or iron *at ordinary temperatures*, failed to effect the substitution of either metal for the mercury in the compound. Prolonged contact with each of the two last-mentioned metals caused pectization, the metal subsequently becoming encrusted with a white substance. Heat produced the same result more rapidly.

When the anhydrous compound was cautiously heated, a quantity of acetone distilled over, and, as the temperature rose, empyreumatic products and mercury were evolved. When the heat was suddenly applied, much metallic mercury distilled over, and a minute quantity of a liquid, having the odour of mercuric methide, was produced, together with carbonic anhydride and other gaseous bodies not particularly examined. If

mercuric methide be formed during the rapid decomposition of the mercuric compound, the first step in the reaction by which it is produced may be explained by the following equation :—



The mercuric methide produced in the first instance, probably at another stage suffers nearly complete decomposition in presence of the oxygen liberated at the outset of the reaction.

d. *Chemical nature of the Diaceto-mercuric Hydrate.*—In the preceding section the properties of the colloid liquid and other bodies obtained by dialysis from the potass solution have been described; but the potassium hydrate used in the first instance may be replaced by solution of sodium, barium, or even calcium hydrate, and yet the same ultimate result arrived at. When sodium hydrate is employed, no material difference is observed at any stage of the operations, but when barium hydrate is substituted, re-solution of mercuric oxide in presence of acetone is quickly effected with the aid of heat; but this alkaline solution slowly decomposes, yielding a white precipitate of the mercuric ketone compound, mixed with a little barium carbonate. This decomposition takes place in closed vessels, and most rapidly when the solution has been boiled in course of preparation, and when no excess of barium hydrate has been employed beyond the amount absolutely required to secure the retention of the mercuric compound in solution.

Keeping in view the peculiar mode of generation of the mercuric ketone compound, its solubility in alkaline liquids at the time of its formation, and insolubility without decomposition in acid solutions, its power of uniting with water to form both liquid and gelatinous hydrates, and the extremely close analogy of these in properties and relations to the “co-silicic acids” of Prof. Graham, we are compelled to attribute to the new hydrates very feeble acid functions.

The alkaline solutions above referred to may therefore be regarded as containing metallic salts of a peculiar acid, derived from the compound $((\text{CH}_3)_2\text{CO})_2\text{Hg}_3\text{O}_3$, already described. That these salts are, however, even more easily decomposed than alkaline silicates is shown :—

1st. By the easy decomposition of the potassium salt by the process of liquid diffusion. The osmotic force alone is sufficient for this purpose, the high diffusive energy of potassium hydrate enabling it to rapidly pass through the dialytic septum, leaving behind the colloid acid in the liquid state. The analogous decomposition occurs somewhat less readily in the case of potassium silicate.

2nd. By the facility with which the new acid may be displaced from the aqueous solutions of its potassium, sodium, or barium salts by so feeble an agent as carbonic acid. Solutions of alkaline silicates are well known to decompose in the same manner, but less rapidly.

3rd. By the fact that heat alone is able to effect the decomposition of the potassium salt, a solution of the latter giving a yellowish-white precipitate of the anhydrous mercuric ketone compound *on violent ebullition*, the alkali remaining dissolved*. Re-solution does not take place on cooling, or on digestion with an excess of the metallic hydrate at the ordinary temperature.

It is, however, worthy of note here that the gelatinous precipitate produced by acetic acid in a solution of the potassium salt is soluble in excess of potassium hydrate *immediately after its formation*; but it soon loses this property and alters somewhat in appearance, becoming more dense.

With a view to obtain, if possible, some evidence of the basicity of the acid, a quantity of mercuric chloride was dissolved in water, the theoretical proportion of pure acetone added, and excess of potassium hydrate. Complete re-solution of the mercuric oxide was obtained as usual. To the alkaline liquid dilute hydrochloric acid was added, until a moderate quantity of the white mercuric-acetone compound had been precipitated. The whole was then filtered as clear as possible. The filtrate now contained, in addition to potassium chloride resulting from the reaction, a certain amount of the mercuric-acetone compound, held in solution by a *minimum* of alkali. In order to determine the ratio between the anhydride or acid and potassium present in combination, 100 cub. centims. of this solution were now taken, treated with excess of hydrochloric acid, and the mercury precipitated as sulphide by means of sulphuretted hydrogen. The pure mercuric sulphide obtained in this manner weighed 1.056 gramme: this amount represents 1.1591 gramme of the anhydrous mercuric-acetone compound, as calculated from the formula already found for that body.

Another 100 cub. centims. of the same solution were taken, and dilute sulphuric acid of known strength very cautiously added, until a drop of the liquid faintly reddened blue litmus-paper: 5.4 cub. centims. of acid were required; this amount of acid represented .2106 gramme of potassium.

100 cub. centims. of the solution, completely free from *excess* of alkali, therefore contained of

	gram.
$(\text{CO}(\text{CH}_3)_2)_2\text{Hg}_3\text{O}_3$	1.1591
K'2106

These numbers, when divided in the usual way by the respective atomic weights, gave the proportion of 1 : 3.6.

The foregoing experiment is but one of many performed with a similar result, the main ratio found for different solutions being 1 : 3.7. When the conditions under which the determinations were made are considered, the ratio 1 : 4 may be admitted as the true result.

* In this, as in many other respects, a *strong* solution closely resembles in deportment a liquid containing *white of egg*.

A solution of the barium salt was now prepared with great care, the presence of excess of barium hydrate being guarded against by very cautious manipulation in the first instance, and subsequent treatment with acetone and mercuric chloride, until the liquid ceased to dissolve more mercuric oxide. The acid treatment resorted to in the case of the potassium salt was found to be unsuitable in the present instance: 100 cub. centims. of the clear filtered liquid were taken, immediately after preparation of the solution, and treated with excess of hydrochloric acid until complete decomposition was effected; the mercury was then precipitated as sulphide: .5062 gramme was obtained; this amount represents .5555 gramme of the anhydrous mercuric-acetone compound.

100 cub. centims. were treated with a standard acid: 3.3 cub. centims. were required before an acid reaction was developed; this corresponds to .2244 gramme of barium.

We therefore find in 100 cub. centims. of the solution of

$(\text{CO}(\text{CH}_3)_2)_2\text{Hg}_2\text{O}_2$	gram. .5555
Ba''2244

When these numbers are treated as before, we find that the ratio of anhydrous ketone compound to barium is 1 : 1.84. As in the case of the potassium salt, the chances of error are altogether in the direction of under-estimating the barium and over-estimating the remaining constituent of the salt; the ratio 1 : 2 may therefore fairly be taken as the practical result of these determinations.

Potassium being monovalent and barium divalent, it would appear that the solutions above mentioned contained respectively the normal potassium and barium salts of an *extremely feeble* but yet distinctly marked tetrabasic acid. When any one of these liquids was evaporated to dryness *in vacuo* over sulphuric acid, a resinoid mass was in each case obtained, from which metallic chloride was removable by water; but since partial decomposition appeared to take place during the process of evaporation in each case, the now insoluble resinoid body, containing potassium, sodium, or barium, could not be regarded as a pure salt; nor have I succeeded in obtaining any other solid compound of this acid in a condition suitable for analysis.

When to a liquid containing the potassium salt of the acid a solution of potassium hydrate, saturated with zinc hydrate, was added, a semi-transparent gelatinous precipitate was obtained. On washing with water this substance quickly became basic. Other attempts with various metals did not afford better results, precipitates of variable composition being obtained in each case. It would appear, then, that only the most powerful soluble metallic hydrates are capable of forcing the new acid to remain in combination, and that even these alkaline salts are so feebly held together, that decomposition attends the attempt to obtain them in the solid state.

If we admit the existence of these salts in the solutions examined, they might be named *di-aceto-mercurates*, or probably *di-keto-mercurates*, the latter name being used as a general term.

e. *Detection of Acetone in the "Methylated Spirit" of Commerce.*—Since acetone is, according to my experience, invariably present in the wood-spirit of commerce, the reaction with mercuric oxide in presence of potassium hydrate, already described, becomes virtually a test for the presence of pyroxilic spirit in any mixture. The ordinary "methylated spirit" of commerce is such a mixture, and the acetone present in it can be detected with great facility in the following way:—Take 200 cub. centims. of the spirit and rapidly distil off 50 cub. centims.; dilute the distillate with an equal volume of water, and slightly warm with addition of a few cub. centims. of solution of potassium hydrate. On cautious addition of mercuric chloride, the oxide first thrown down is speedily redissolved: excess of the mercuric salt must be carefully avoided. The alkaline liquid should be filtered clear, much of the alcohol allowed to evaporate slowly, and the residue then divided in two portions. One part is to be violently boiled for a few minutes; a yellowish-white gelatinous precipitate will suddenly make its appearance, if the acetone compound be present. In the second portion, dilute acetic acid, when added in slight excess, should produce a bulky, white, gelatinous precipitate containing, when washed and completely dried, between 78 and 79 per cent. of mercury.

Finally, by means of the above test the admixture of "methylated spirit" with alcoholic solutions may be detected with facility. In all such cases, however, a specimen of a pure alcoholic solution should be tested at the same time as the suspected sample, distillation of considerable quantities of the liquids being resorted to in every instance.

2. *Bodies containing Mercury and higher members of the Fatty Ketone series.*—Experience has proved that several ketones of the fatty series are capable of uniting with mercuric oxide in the presence of alkali to form compounds analogous to that obtained with acetone. The higher ketones being insoluble in water though soluble in alcohol, it was found to be extremely difficult to prepare the colloidal hydrates or hydrosols of the mercuric compound.

In the case of butyrone, however, I have succeeded completely in obtaining a hydrosol.

The general reactions of the several solutions leave no doubt that the compound contained in each belongs to the group of colloid bodies and not to that of crystalloids.

The following results have been obtained:—

(a) *With Propione.*—The rectified product of the careful destructive distillation of barium propionate was dissolved in alcohol and mercuric chloride added; excess of strong aqueous solution of potassium hydrate was then poured in; on gently warming the mixture the mercuric oxide dissolved in great part. The liquid, filtered as clear as possible, was found

to be rich in mercury, and when treated with excess of dilute acetic acid, gave a white gelatinous precipitate precisely analogous to that obtained under similar circumstances from the potassium di-aceto-mercurate.

The precipitate was found on analysis to contain 72·26 per cent. of mercury; the formula requires 73·17 per cent. On treatment with hydrochloric acid, the precipitate afforded mercuric chloride and an oily liquid, insoluble in water, possessing the odour of and characters attributed to propione.

(b) *With Butyrone*.—The rectified product of the destructive distillation of calcium butyrate, when dissolved in alcohol and treated with mercuric chloride and potassium hydrate, gave, on warming, a liquid similar to that obtained with propione. Like the latter, it afforded a white precipitate on treatment with excess of dilute acetic acid. The precipitate, when carefully washed and dried, contained 69·1 and 69·27 per cent. of mercury, the formula $((C_3H_7)_2CO)Hg_2O$, requiring 68·49 per cent. The freshly prepared precipitate, when digested with excess of hydrochloric acid, afforded mercuric chloride and a small quantity of a liquid insoluble in water and recognizable as butyrone.

The alkaline alcoholic solution of the butyrone mercuric compound was diluted with its own volume of water, then filtered and carefully warmed in order to slowly evaporate the alcohol as much as possible. When a considerable portion of the latter had been removed in this way, the clear residual liquid was placed on a dialyzer, and the attempt made to diffuse away the crystalloids from the liquid. After treatment for five days, most of the chloride had diffused away, and a liquid was obtained which peptized on heating to near the boiling-point and on the addition of a small quantity of a mineral acid. The precipitate first formed by hydrochloric acid was easily redissolved on heating with excess of the reagent, and the odour of the ketone developed. The attempt to convert the alcocol of the mercuric compound into the hydrosol had therefore been attended with success.

(c) *With Valerone*, obtained by purifying the product of the distillation of calcium valerate previously mixed with one-sixth of its weight of quicklime. When this was dissolved in alcohol and the solution was rendered alkaline by potassium hydrate and mercuric chloride dropped in, the mercuric oxide first precipitated was speedily redissolved; but from the solution no precipitate was thrown down on addition of an acid previously mixed with alcohol. I have not obtained any compound from this alkaline liquid possessing distinctive properties. It is not improbable that the valerone mercuric compound, if formed, is more easily decomposed by acids than the corresponding bodies obtained with other ketones; hence the difficulty of isolating it.

3. *Depoiment of certain Aldehydes in presence of Mercuric Oxide and Alkaline Metallic Hydrate*.—The close similarity in chemical relations be-

tween the members of the groups of ketones and of aldehydes suggested the attempt to combine the latter bodies with mercuric oxide. Though the experiments in this direction cannot properly be detailed in this communication, I may be permitted to state that when the aldehyde of the ethyl series was warmed with freshly precipitated mercuric oxide in presence of potassium hydrate, solution of the oxide did not take place, but the latter gradually united with the aldehyde to form a white non-crystalline compound. I have not obtained any colloid liquid in the course of my experiments with this body.

Certain other aldehydes of the same series were found to unite with mercuric oxide, yielding soluble compounds easily decomposed by weak acids.

The investigation of the bodies produced in these reactions has been attended with peculiar difficulty, owing, on the one hand, to the facility with which several aldehydes are converted into resinoid products of variable composition by contact with caustic alkali; and, on the other, to the circumstance that an aldehyde can react either as the oxide of a divalent group ($C_nH_{2n}''O$), or as the hydride of a monovalent acid radical ($C_nH_{2n-1}O$, H, or, for terms of the series above methylic aldehyde, we may write the formula $CO(C_nH_{2n-1})H$). It may be added, that the well-known tendency of the aldehyde to yield polymeric compounds has in no degree lessened the uncertainty attending the examination of the products of the action of mercuric oxide and alkali upon the members of the group.

Leaving the statements contained in the last section out of consideration for obvious reasons, I may now be allowed to summarize the results of the investigation detailed in the foregoing pages. It has been shown:—

1st. That certain fatty ketones can be made to unite directly with mercuric oxide.

2nd. That the resulting compounds afford a new group of colloid hydrates, analogous in properties to the silicic, albuminic, and other hydrates already made known by the researches of Professor Graham.

3rd. That the new hydrates may best be regarded as extremely feeble conjugate acids, the chief member of the group (that derivable from acetone) being probably tetrabasic and capable of affording very unstable salts.

4th. That the generating reaction for the di-keto-mercurates may, when carefully controlled, be employed as a test for the presence of a ketone (especially for acetone) in certain organic mixtures.

April 27, 1871.

General Sir EDWARD SABINE, K.C.B., President, in the Chair.

THE BAKERIAN LECTURE was delivered by CHARLES WILLIAM SIEMENS, F.R.S., D.C.L., "On the Increase of Electrical Resistance in Conductors with rise of Temperature, and its application to the Measure of Ordinary and Furnace Temperatures; also on a simple Method of measuring Electrical Resistances." The following is an Abstract.

The first part of this Paper treats of the question of the ratio of increase of resistance in metallic conductors with increase of temperature.

The investigations of Arndtson, Dr. Werner Siemens, and Dr. Matthiessen are limited to the range of temperatures between the freezing- and boiling-points of water, and do not comprise platinum, which is the most valuable metal for constructing pyrometric instruments.

Several series of observations are given on different metals, including platinum, copper, and iron, ranging from the freezing-point to 350° Cent.; another set of experiments being also given, extending the observations to 1000° Cent. These results are planned on a diagram, showing a ratio of increase which does not agree either with the former assumption of a uniform progression, or with Dr. Matthiessen's formula, except between the narrow limits of his actual observations, but which conforms itself to a parabolic ratio, modified by two other coefficients, representing linear expansion and an ultimate minimum resistance.

In assuming a dynamical law, according to which the electrical resistance of a conductor increases according to the velocity with which the atoms are moved by heat, a parabolic ratio of increase of resistance with increase of temperature follows; and in adding to this the coefficients just mentioned, the resistance r for any temperature is expressed by the general formula

$$r = \alpha T^{\frac{1}{2}} + \beta T + \gamma,$$

which is found to agree very closely both with the experimental data at low temperatures supplied by Dr. Matthiessen, and with the author's experimental results, ranging up to 1000° Cent. He admits, however, that further researches will be necessary to prove the limits of the applicability of the law of increase expressed by this formula to conductors generally, especially when nearing their fusing-point.

In the second part of this Paper it is shown that, in taking advantage of the circumstance that the electrical resistance of a metallic conductor increases with an increase of temperature, an instrument may be devised for measuring with great accuracy the temperature at distant or inaccessible places, including the interior of furnaces, where metallurgical or other smelting-operations are carried on.

In measuring temperatures not exceeding 100° Cent., the instrument is so arranged that two similar coils are connected by a light cable containing three insulated wires. One of these coils, "the thermometer-coil," being carefully protected against moisture, may be lowered into the sea, or buried in the ground, or fixed at any elevated or inaccessible place whose temperature has to be recorded from time to time; while the other, or "comparison-coil," is plunged into a test-bath, whose temperature is raised or lowered by the addition of hot or cold water, or of refrigerated solutions, until an electrical balance is established between the resistances of the two coils, as indicated by a galvanoscope, or by a differential voltameter, described in the third part of the paper, which balance implies an identity of temperature at the two coils. The temperature of the test-solution is thereupon measured by means of a delicate mercury thermometer, which at the same time tells the temperature at the distant place.

By another arrangement the comparison-coil is dispensed with, and the resistance of the thermometer-coil, which is a known quantity at zero temperature, is measured by a differential voltameter, which forms the subject of the third part of the paper; and the temperature corresponding to the indications of the instrument is found in a table, prepared for this purpose, in order to save all calculation.

In measuring furnace temperatures the platinum-wire constituting the pyrometer is wound upon a small cylinder of porcelain contained in a closed tube of iron or platinum, which is exposed to the heat to be measured. If the heat does not exceed a full red heat, or, say, 1000° Cent., the protected wire may be left permanently in the stove or furnace whose temperature has to be recorded from time to time; but in measuring temperatures exceeding 1000° Cent., the tube is only exposed during a measured interval of, say, three minutes, to the heat, which time suffices for the thin protecting casing and the wire immediately exposed to its heated sides to acquire within a determinable limit the temperature to be measured, but is not sufficient to soften the porcelain cylinder upon which the wire is wound. In this way temperatures exceeding the welding-point of iron, and approaching the melting-point of platinum, can be measured by the same instrument by which slight variations at ordinary temperatures are told. A thermometric scale is thus obtained embracing without a break the entire range.

The leading wires between the thermometric coil and the measuring instrument (which may be, under certain circumstances, several miles in length) would exercise a considerable disturbing influence if this were not eliminated by means of the third leading wire before mentioned, which is common to both branches of the measuring instrument.

Another source of error in the electrical pyrometer would arise through the porcelain cylinder upon which the wire is wound becoming conductive at very elevated temperatures; but it is shown that the error arising through

this source is not of serious import.

The third part of the paper is descriptive of an instrument for measuring electrical resistance without the aid of a magnetic needle or of resistance scales. It consists of two voltameter tubes fixed upon graduated scales, which are so connected that the current of a battery is divided between them, with one branch including a known and permanent resistance, and the other the unknown resistance to be measured. The resistance and polarization being equal, and the battery being common to both circuits, these unstable elements are eliminated by balancing them from the circulation; and an expression is found for the unknown resistance X in terms of the known resistances C and γ of the voltameter, including the connecting-wires, and of the volumes V and V' of gases evolved in an arbitrary space of time within the tubes, viz. :—

$$X = \frac{V}{V'}(C + \gamma) - \gamma. \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Changes of atmospheric pressure affect both sides equally, and do not therefore influence the results; but a reading at the atmospheric pressure is obtained at both sides by lowering the little supply-reservoir with dilute acid to the level indicated in the corresponding tube. The upper ends of the voltameter tubes are closed by small weighted levers provided with cushions of India-rubber; but after each observation these levers are raised, and the supply-reservoirs moved so as to cause the escape of the gases until the liquid within the tubes is again brought up to the zero-line of the scale, when the instrument is ready for another observation. A series of measurements are given of resistances varying from 1 to 10,000 units, showing that the results agree within one-half per cent. with the independent measurements obtained of the same resistances by the Wheatstone method.

The advantages claimed for the proposed instrument are, that it is not influenced by magnetic disturbances or the ship's motion if used at sea, that it can be used by persons not familiar with electrical testing, and that it is of very simple construction.

The following communications were read :—

- I. "On the Change of Pressure and Volume produced by Chemical Combination." By M. BERTHELOT. Communicated by Dr. WILLIAMSON. Received April 25, 1871.

1. A singular question has arisen in the study of the gaseous combinations, viz. can the pressure be diminished in consequence of a reaction, at the moment it is accomplished, at constant volume, without loss of heat, so that the phenomenon of explosion comes from the excess of atmospheric pressure upon the inner pressure of the system, instead of coming from the inverse excess of the inner pressure? The discussion of this question, however special it appears at first sight, leads to general notions concerning chemical combination.

2. The pressure depends upon the temperature evolved, and upon the state of condensation of the products. Let us determine this quantity.

Let t be the temperature produced by the real reaction, this being effected at a constant volume, admitting that the whole of the disengaged heat was employed in warming the products.

Let V be the sum of the volumes of the gaseous bodies in the initial system at 0° and $0^{\text{m}}\cdot 760$.

At the temperature t , the final system contains in general certain gaseous bodies.

Let V' be the volume of these bodies, supposed to be brought, without changing their state, to 0° and $0^{\text{m}}\cdot 760$.

The relation $\frac{V'}{V} = \frac{1}{K}$ expresses the condensation produced by the reaction.

When certain bodies, contained in the initial system at 0° , or in the final system at t° , are in the solid or the liquid state, you can generally neglect their volume in comparison with that of the gas, when the pressures are not too considerable. Let us calculate the pressure during the reaction which takes place at a constant volume and at the temperature t , the initial temperature and pressure being 0° and H .

In admitting Marriotte's and Gay-Lussac's laws, the pressure will become

$$H \times \frac{1}{K}(1 + \alpha t);$$

it will be greater than the initial pressure if $1 + \alpha t > K$, less if $1 + \alpha t < K$, or equal if $1 + \alpha t = K$. Let us observe that $t = \frac{Q}{c}$, Q being the quantity of heat produced in the reaction, and c being the mean specific heat of the products between 0° and t° .

Let us developpe this solution.

3. The pressure augments when the condensation is null, for instance chlorine and hydrogen, $K=1$; and especially when there is dilatation (combustion of acetylene by oxygen), t being always positive in a direct and rapid reaction between gaseous bodies.

4. On the contrary, the pressure diminishes if K is very great—that is, in the case of a system containing gaseous bodies transformed *entirely* into products which are in the solid or liquid state at the *temperature developed by the reaction*. This case is more rare than one would think at first sight, because very few compounds subsist wholly at the high temperature that would be developed by the integral union of their gaseous components. Generally a portion of these remain free at the moment of the reaction; but in the present state of our knowledge it is impossible to estimate the pressure corresponding to effects so complex.

It is necessary to consider that the present case must not be confounded with the case in which the products formed in the gaseous state and at the *temperature of the reaction* are liquefied or solidified under the influence

of a subsequent cooling; for instance, the formation of water from its elements, or of chlorhydrate of ammonia from hydrochloric acid and ammonia, produce equally a diminution in the final pressure.

5. In theory the most interesting case is that in which the initial and final systems are wholly constituted of gaseous bodies, whose volume (calculated at 0° and $0^m\cdot760$) is more condensed in the final than in the initial system. But this condensation is always comprised within very narrow limits, such as $K=4$ (formation of arsenious acid by the elements), $K=3$, 2, $1\frac{1}{2}$, &c.; so the fundamental condition

$$1 + \alpha \frac{Q}{c} < K, \text{ or } Q < 273 (K - 1) c,$$

that determines a diminution of pressure, should be realized only in very exceptional cases and when the heat evolved by an integral reaction is very little.

One can ascertain it in making the calculation by means of the specific heats at constant volume (deduced with ordinary coefficient from the specific heats at constant pressure which M. Regnault has determined for many bodies). One can also make the calculation in a more general manner, in admitting with Clausius that the specific heats at constant volume have an identical value for the atomic weights of elements, that this value is equal to 2, 4, the number found for $H=1$, and that it does not change from the fact of combination. W being the quantity of heat produced in a reaction between gaseous bodies calculated for atomic weights, and n the number of atoms in the reaction, the pressure will diminish only if

$$W < 655n (K - 1).$$

It is easy to see that this condition is not fulfilled in the combinations best known. Calculating, either by means of this formula or by means of the preceding, I have not succeeded in discovering any example of diminution of pressure among the numerous reactions I have examined in this present research.

Besides, it is sufficient to make the calculation for the reaction supposed integral, the conclusion being generally the same for the reaction supposed incomplete, that is to say in the case of dissociation, as it would be easy to prove.

6. Without further extending this discussion, I believe that a new general proposition relative to chemical combination can be deduced from it. It is known that every direct reaction which can be accomplished in a very short time between gaseous bodies with formation of gaseous compounds, produces a disengagement of heat: this is true for all reactions evolved by chemical forces alone, acting without help of any work done by exterior forces*. The new proposition is the following:—

The heat produced in a reaction of this sort, supposing it to be applied

* This proposition is contained in a more general one, which I have given in 'Annales de Chimie et Physique,' 4^{me} série, t. xviii.

exclusively and without any loss to warm the products, is such that an augmentation of pressure always takes place at a constant volume, or, what is the same thing, an augmentation of volume at a constant pressure.

This proposition results not from any *à priori* deduction, but is verified by the whole of facts known to this day.

7. One may ask if the change of volume, in which the gases keep the whole heat produced by their mutual actions, is regulated by a simple law, analogous to those that have been observed when the gaseous combinations are brought to the same temperature; nevertheless it does not appear to be so.

Let us compare the formation of the different hydracids by means of their gaseous elements, which gives no change of volume when the gas is reduced at 0° and $0^{\text{m}}\cdot760$.

The formation of chlorhydric gas, H Cl , produces 23,900 calories; the formation of bromhydric gas, H Br , produces 13,400 calories; the formation of iodhydric gas, HI , produces 800 calories. The specific heat of these gases being nearly the same under the same volume, it is clear that the quantities of heat aforesaid cannot produce an augmentation of volumes identical or proportional with simple numbers.

II. "Remarks on the Determination of a Ship's Place at Sea." In a Letter to Prof. STOKES. By G. B. AIRY, LL.D., &c., Astronomer Royal.

Royal Observatory, Greenwich, S.E.,
1871, April 5.

MY DEAR SIR,—In the last published Number of Proceedings of the Royal Society*, there are remarks by Sir William Thomson on the proposed method for determining the *locus* of a ship's place at sea, by making one observation of the sun's (or other body's) altitude, and founding, on this, computations of longitude with two assumptions of latitude; and there are suggestions, with a specimen of tables, for solving the spherical triangles which occur in all similar nautical observations, on the principle of drawing a perpendicular arc of great circle from one angle of a spherical triangle upon the opposite side.

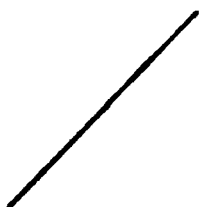
In regard to this principle and the tables which may be used with it, I may call attention to the employment of a similar method by Major-General Shortrede, in his 'Latitude and Declination Tables,' pp. 148 and 180. In p. 150, line 11 from the bottom, it will be seen that the "column" gives the trial-value of the perpendicular arc by which the two right-angled triangles are computed. This is not the same (among the various elements which may be chosen) as Sir William Thomson's; but it is so closely related that in some instances the tabular numbers are identically the same as Sir W. Thomson's, though in a different order. General Shortrede's

* Vol. xix. p. 259.

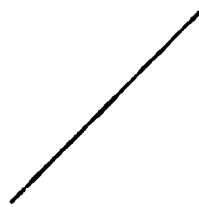
object was "Great Circle Sailing," in which the trigonometrical problem is the same as in the nautical observation. I think, however, that Sir W. Thomson deserves thanks for calling attention to the application of this method to time-determinations.

In regard to the problem of the "locus," allow me to point out the geometrical circumstances of the case. If, upon a celestial globe, an arc of small circle be swept with the sun's (or other body's) place for centre, and the observed zenith-distance for radius, the ship's zenith will be somewhere in that curve; and if, with the pole for centre, arcs of parallels be swept with the two assumed colatitudes for radii, the intersection of these two curves with the first drawn curve will give the ship's zenith on the two assumptions; and if within the celestial globe there be placed a small terrestrial globe, and if these zenith-points be radially projected upon the terrestrial globe, the terrestrial places of the ship on the two assumptions will be marked. But the practical application of this requires that the position of the terrestrial globe, or of the earth, be known in respect of rotation,—that is, it requires that the Greenwich sidereal time, or solar time, be known; in other words, it requires a perfect chronometer. Now the experience of Captain Moriarty, cited by Sir W. Thomson, does not apply here. Captain Moriarty received time-signals from the Royal Observatory through the cable every day, and he had therefore a perfect chronometer. But other ships have no such perfect chronometer; and though the *direction* of a *locus*, as determined above, may be sufficiently certain, yet its *place upon the earth* will be uncertain, by a quantity depending on the uncertainty of the chronometer. Thus three chronometers may give the following positions for the locus-curve:—

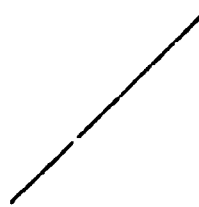
Chron. No. 1.



Chron. No. 2.



Chron. No. 3.



And the question now presents itself, which uncertainty is the greater,—the uncertainty of latitude, which it is the real object of this problem to remedy? or the uncertainty of the chronometric longitude, which must be used in attempting to find the remedy? I do not doubt the instant reply of any practical navigator, that the chronometric longitude is far more uncertain than the latitude; and if it be so, the whole method falls to the ground.

I fear that a publication like that which has been given to this method may do very great injury among navigators who are not accustomed to investigate the geometrical bearings of such operations, and may lead them into serious danger.

I am, my dear Sir, yours very truly,

G. B. AIRY.

Professor Stokes, Secretary of the Royal Society.

[From a general recollection of a conversation I had with Sir W. Thomson before the presentation of his paper, I do not imagine his object to have been exactly what the Astronomer Royal here describes, but partly the saving of trouble in numerical calculation, partly the exhibition, for each separate observation of altitude at a noted chronometer time, of *precisely what that observation gives, neither more nor less*, which introduces at the same time certain facilities for the determination of a ship's place by a combination of two observations. Of course the place so determined is liable to an error east or west corresponding to the unknown error of the chronometer; and doubtless, under ordinary circumstances, this forms the principal error to which the determination of a ship's place is liable. This remains precisely as it did before; and it is hard to suppose that the mere substitution of a graphical for a purely numerical process could lead a navigator to forget that he is dependent upon his chronometer, though perhaps the general tone of Sir W. Thomson's paper might render an explicit warning desirable, such as that which Mr. Airy supplies.—G. G. STOKES.]

May 4, 1871.

Sir PHILIP GREY-EGERTON, Bart., Vice-President, in the Chair.

In conformity with the Statutes, the names of the Candidates recommended for election into the Society were read from the Chair, as follows :—

William Henry Besant, M.A.
 William Budd, M.D.
 George William Callender, F.R.C.S.
 William Carruthers, Esq.
 Robert Etheridge, F.R.S.E.
 Frederick Guthrie, B.A.
 John Herschel, Capt. R.E.
 Alexander Moncrieff, Capt. M.A.

Richard Quain, M.D.
 Carl Schorlemmer, Esq.
 Edward Thomas, Esq.
 Edward Burnet Tylor, Esq.
 Cromwell Fleetwood Varley, C.E.
 Arthur Viscount Walden, P.Z.S.
 John Wood, F.R.C.S.

The following communications were read :—

- I. "On the Structure and Affinities of *Guynia annulata*, Dunc., with Remarks upon the Persistence of Palæozoic Types of *Madreporaria*." By P. MARTIN DUNCAN, M.B. Lond., F.R.S., Professor of Geology in King's College, London. Received March 16, 1871.

(Abstract.)

The dredging-expedition which searched the sea-floor in the track of the Gulf-stream of 1868, yielded, amongst other interesting *Madreporaria*, a form which has been described by Count Pourtales under the name of

Haplophyllia paradoxa, and which was decided by him to belong to the section *Rugosa*.

The last expedition of the 'Porcupine,' under the supervision of Dr. Carpenter, F.R.S., and Mr. J. Gwyn Jeffreys, F.R.S., obtained, off the Adventure Bank in the Mediterranean, many specimens of a coral which has very remarkable structures and affinities. The species is described under the name of *Guynia annulata*, Dunc. The necessity of including it amongst the *Rugosa* and in the same family, the *Cyathoxonidæ*, as *Haplophyllia paradoxa* is shown.

Having this proof of the persistence of the rugose type from the Palæozoic seas to the present, the affinities of some so-called anomalous genera of Midtertiary and Secondary deposits are critically examined. The Australian tertiary genus *Conosmilia*, three of whose species have strong structural resemblance with the *Rugosa*, is determined to be allied to the *Stauridæ*, and especially to the Permian genus *Polycælia*. The Secondary and Tertiary genera with hexameral, octomeral, or tetrameral and decameral septal arrangements are noticed, and the rugose characteristics of many lower Liassic and Rhetic species are examined.

The impossibility of maintaining the distinctness of the Palæozoic and Neozoic coral-faunas is asserted; and it is attempted to be proved that whilst some rugose types have persisted, hexameral types have originated from others, and have occasionally reverted to the original tetrameral or octomeral types, and that the species of corals with the confused and irregular septal members so characteristic of the lowest Neozoic strata descended from those *Rugosa* which have an indefinite arrangement of the septa.

The relation between the Australian Tertiary and recent faunas, and those of the later Palæozoic and early Neozoic in Europe, is noticed, and also the long-continued biological alliances between the coral-faunas of the two sides of the Atlantic Ocean.

II. "On the Molybdates and Vanadates of Lead, and on a new Mineral from Leadhills." By Professor Dr. ALBERT SCHRAUF, of Vienna. Communicated by Professor W. H. MILLER, For. Sec. R.S. (Translated from the Author's Manuscript by Count A. FR. MARSHALL.) Received March 9, 1871.

In 1825*, Professor Wöhler published the analysis of a rare variety of pyromorphite from Leadhills, which he describes as an aggregation of very diminutive, orange-red hexagonal crystals, fixed on cerussite. The normal constituents of pyromorphite are mixed in them with small quantities of iron and arsenic. Professor Wöhler having ascertained the existence of vanadate of lead only five years later (1830), and the presence of partly

* Poggendorff's Ann. vol. iv. p. 160.

chromium, partly vanadium in Russian specimens of pyromorphite having been stated, it may be supposed that the red pyromorphite from Leadhills owes its characteristic coloration to one of the two elements named before.

An examination of a great number of cabinet specimens from Leadhills gave no satisfactory result. I could not find any red pyromorphite, as described by Professor Wöhler, on the specimens at my disposal, and must consequently leave unproved the above-stated supposition concerning the colouring-substance of red pyromorphite. A cabinet specimen, dating from the years 1820–1825, gave me, however, the proof that the specimens of cerussite found at Leadhills during this period were not completely free from an admixture of molybdenum and vanadium. I succeeded in finding out a specimen of crystallized “vanadine-molybdate of lead” from Leadhills so strikingly different in crystallographical and chemical characters from its nearest allies, wulfenite and descloizite, that I am authorized to consider it as the type of a new species, for which I propose the name of “eosite,” alluding to its saturated aurora-red colour.

§ 1. *Paragenetic relations of Eosite.*

The cabinet specimen on which I have found the crystals of eosite has been in possession of the Imperial Museum since 1828; it must therefore, as remarked above, date from the years between 1820 and 1825. Its matrix, abundantly beset with crystals of cerussite, is cellular, ochreous galena. The crystals of cerussite, about 3–8 millimetres in size, bear the aspect of plates, and are greenish yellow; some of them completely fill up a deep cavity in the ochreous matrix, others are scattered on the surface of the specimen. This cerussite, as also some parts of the matrix, is in several points covered with small moss-like aggregations of delicate and minute acicular yellow crystals. If such a fascicular aggregation is detached, and the concentrically grouped acicular crystals taken away, their nucleus is found to be a very small red crystal fixed on cerussite. When examined with a powerful magnifying-lens, the specimen shows about twenty such minute red crystals more or less scattered over the surface of the cerussite, wrapped up, some of them entirely, others only by half, in the above-described fine yellow acicular crystals. As was subsequently proved by precise determination, the minute red octahedra are eosite, and the yellow acicular crystals are pyromorphite.

These last crystals, being only $\frac{1}{2}$ –1 millimetre in length and about $\frac{1}{10}$ millimetre in thickness, could only be determined by the aid of the microscope.

They are yellow, pellucid or transparent, very brilliant, with light-yellow streaks. The microscope shows prismatic planes without any distinguishable terminal planes. As previously for my investigations on labradorite, I used for the measurement of the angles of the prismatic planes a vertical circle adapted above the horizontal table of the microscope, whose axis bears the object to be measured beneath the microscope's focus;

the crystal fixed to the axis being duly adjusted and centred, the proceedings go on as usual. Two acicular crystals, duly adjusted, gave as results :

0°,
60°,
120°,
180°,

from which the planes, including them, must be admitted to be those of a regular hexagonal prism.

Their colour, and even their streak, being light yellow, they may be either mimesite or pyromorphite on vanadate of lead. A drop of hydrochloric acid poured on such a crystal makes its tint vanish gradually without bringing to view a dark-brown nucleus (as is the case with vanadate of lead), and finally produces a pseudomorph of white chloride of lead. By heating on charcoal, a brown globule with faceted surfaces is produced ; traces of lead are obtained ; the smell of arsenic is very insignificant. Fusing with bisulphate of potash in a platinum spoon gives merely a portion of white saline substance. All these tests were indicative of pyromorphite without any admixture of vanadium ; the quantities submitted to operation being, however, very minute, a definitive judgment is not to be pronounced.

§ 2. *Chemical Characters of Eosite.*

The crystals of this mineral are all below $\frac{1}{4}$ millim. in size, and difficult to be freed from the acicular pyromorphite covering them. They are easily detached from the cerussite, on whose surface they are fixed. Their hardness lies between 3 and 4 (Mohs's scale). When compressed, they are crushed into small, irregular granules, showing slight traces of cleavage.

The colour of the eosite is a saturated aurora-red, even deeper than that of chromate of lead, and approaching the tint of realgar ; the red tint of the wulfenite from Ruskberg (Banat) is less intense, that of the Phenixville specimens has more of yellow in it ; the tints of dechenite and descloizite vary between brownish carnation and greenish brown.

The powder resulting from the friction on a hard surface is brownish orange in eosite, somewhat darker than that from red chromate of lead.

Like the powder from chromate of lead, or from red wulfenite, that from eosite loses its colour and becomes white by contact with hydrochloric acid. This solution, diluted and spread over a glass plate, shows under the microscope the formation of chloride of lead in white acicular crystals. A minute splinter of eosite, when put on a glass plate and a drop of cold hydrochloric acid poured on it, undergoes, within a quarter of an hour, a partial solution along its margins, pseudomorphizing into white chloride of lead, the interior still remaining partly unaltered. The solution in hydrochloric acid is slightly tinted with yellow ; chromate of lead and vanadinite are more easily dissolved, and the tints of their solutions are more

intense. Wulfenite, even when used in fragments ten times larger, loses its colour and becomes white in far shorter time.

If a splinter of eosite is dissolved in a drop of slightly heated hydrochloric acid, the solution spread on a glass plate, some alcohol added to it, and the whole again heated to dryness*, a bluish deposit, somewhat verging into greenish grey, is obtained. With respect to the quantity of eosite experimented upon, and to the extent of the deposit, its tint may be considered as of medium intensity; it is bordered on its margin by a delicate green, somewhat acicular precipitate. Fragments of yellow wulfenite, vanadinite, or red chromate of lead, of equal size and treated in the same way, give different deposits on the glass plate,—wulfenite a deep blue one (on account of the formation of molybdate of molybdenum), vanadinite a yellowish or bluish-green one, and crocoïte a feeble yellowish-green one†. The crystals of eosite being of scarce occurrence and of diminutive size, only few experiments could be made concerning the action of the blowpipe on them.

Eosite, when heated in a glass tube, assumes a darker tint without decrepitating, and, when cooled, takes again its original colour. A splinter of this mineral, mixed with three times its volume of bisulphate of potash and fused on platinum-foil, gives under a glowing heat a transparent, nearly colourless saline mass with a very slight light-yellow tint. During its cooling, this mass becomes for a moment reddish brown, and, after complete cooling, assumes a light orange-brown tint.

Comparative experiments made with chromate, vanadate, and molybdate of lead, as also with binary mixtures of the powders of these three substances, proved the colour of the eosite salt to approach very nearly the tint of the saline mass obtained by fusing a mixture of two to three parts of molybdate of lead and one part of vanadate of lead with bisulphate of potash.

The substance obtained by fusing eosite with bisulphate of potash, brought into contact with water in a platinum spoon, gives a solution which, some tin-foil being added to it and the whole being brought to ebullition‡, assumes a faint greenish-blue tint. A second experiment gave the same result, and proved the solution to give, when further evaporated, a yellowish-brown residuum—possibly caused by the presence of

* It must be remarked that all these experiments have been made under the microscope, or, at least, under a powerful magnifying-lens, so that it was impossible to separate the chloride of lead from the solution containing molybdenum.

† As M. Czudnowicz has stated (Poggendorff's Ann. vol. cxx. p. 17), the action of alcohol on the hydrochloric solution of vanadium gives rise to precipitates tinted between blue or brown, according to the degree of oxidation. I also obtained at first, by mixing with alcohol a solution of vanadate of lead, precipitates changing from yellowish green into brown, according to the degree of heat. The exact and constant temperature for the evaporation of alcohol was, however, soon ascertained by a series of experiments.

‡ The splinters of eosite submitted to chemical action being scarcely $\frac{1}{2}$ millim. in size, the chloride of lead could not be isolated.

vanadium. Wulfenite, treated in the same way, gave a deep blue tint, vanadinite a light yellowish-green, and chromate of lead a light yellowish grey of the saline solution heated in contact with tin-foil.

It would be desirable to ascertain experimentally the presence of other metals besides lead, were not the scarcity of the material an obstacle to any further experiments besides those of absolute necessity.

Considering the whole of the experiments above described, the way in which eosite is acted upon by heating in a glass tube, by hydrochloric acid, alcohol, and bisulphate of potash, proves this mineral to be essentially a vanado-molybdate of lead; possibly with an excess of molybdenum, as it may be supposed from the colorations caused by chemical action, the presence of an undoubtedly minute quantity of chromium being concealed by the chemical action of the prevalent constituents*.

The investigations here detailed having ascertained the presence of lead, molybdenum, and vanadic acid in eosite, it is still to be proved that the chemical actions of vanadate and molybdate of lead are essentially different from those manifested by eosite, and that consequently this mineral is to be identified neither with the red varieties of wulfenite actually known, nor with the crystals of dechenite, vanadinite, or descloizite.

§ 3. *Chemical Properties of the Chromo-Wulfenites.*

The late Professor H. Rose (Poggendorff's Ann. vol. xlvi. p. 639) is known to have investigated the red varieties of wulfenite from Rézbánya (Banat) and Siberia, and to have ascertained the presence of chromium in them. I have before me cabinet specimens from Rézbánya, Ruskberg (Banat), and Phenixville, which I shall comprehend here under the general denomination of "chromo-wulfenites." The specimens from Rézbánya being rather yellowish than pure red, I must confine my investigations to those from Ruskberg and Phenixville.

The matrix of the Ruskberg pyromorphite is cellular quartz† and galena. A good number of rather bright, deep-red octahedral crystals of wulfenite, 1–2 millims. in size, are fixed at the surface of the pyromorphite.

The crystals of Phenixville are notably larger (2–4 millims.), and con- creted into a crust on the surface of the matrix (quartz and pyromorphite). Although apparently of even surface, these crystals show merely a peculiar wax-like brightness.

The red of eosite is deeper than that of crocoïte; it is somewhat more intense than that of the Ruskberg chromo-wulfenite, and less mixed with yellow than in the Phenixville specimens. The streak is browner in

* The existence of vanadium, as accidentally entering into the composition of the genuine wulfenites, was ascertained by Prof. Wöhler on the occasion of his experiments concerning the production of molybdic acid by the treatment of wulfenites (see Liebig and Kopp, Ann. d. Chem. und Pharm. vol. cii. p. 383).

† Ruskberg lies in Austrian Banat, next to the threefold frontier between Banat, Transylvania, and Wallachia.

eosite than in chromate of lead. The streaks of the Ruskberg crystals and of those from Berezowsk are quite identical; the powder of the Phenixville crystals is light orange, verging into sulphur-yellow.

The Ruskberg and Phenixville chromo-wulfenites, treated with hydrochloric acid in the way above described, both leave a deep-blue precipitate with a yellowish-green margin. Fused with bisulphate of potash in a platinum spoon, they both give a saline compound, becoming very faintly yellowish green after cooling. In the beginning of fusion a brownish-purple tint appeared, especially on the Phenixville crystals. This circumstance, not remarked in eosite or vanadate of lead, confirms the fact ascertained by Prof. Rose, of chromium being a prevailing component of the red wulfenites of the Banat, with which, according to my observations, I rank also the yellowish-red wulfenites from Phenixville. According to Mr. Smith, these last contain vanadium; however, the specimen before me, when fused with bisulphate of potash, would certainly have manifested the presence of this metal had it been a prevailing constituent, as chromium really is. At all events, the red wulfenites may possibly contain a certain proportion of vanadium, as may be expected with regard to nearly all wulfenites in consequence of Prof. Wöhler's statements.

§ 4. *Chemical properties of the Vanadates of Lead (Dechenite, Descloizite, and Vanadinite).*

Eosite, as containing vanadic acid, stands next to the just-mentioned vanadates of lead. Dechenite was first found in 1851 by M. Krantz, near Schlettenbach; and M. Bergemann, neglecting a notable proportion of zinc* contained in this mineral, has stated for it the chemical formula VO_3PbO . Vanadinite, first found at Kappel (Carinthia), received its specific name by the late Prof. Zippe, and is, according to M. Tschermak, PbOVO_3 . Descloizite was stated by Mr. Damour (1854) to correspond to the formula $2\text{PbO}, \text{VO}_3$. I have, as early as 1861†, expressed the opinion that these three species, concordant in a great number of properties, may be considered as being specifically identical. M. Czudnowicz (*l. c.*), in his paper on vanadinite, has offered several arguments against the vanadium supposed to enter into the composition of these minerals, so near related to each other that they must be submitted altogether to comparative investigation. The Peruvian descloizite is of so rare occurrence, that I

* See Czudnowicz in Poggend. Ann. vol. cxx.

† See Schrauf, in Poggend. Ann. vol. cxvi. I must here remark expressly, in order to avoid misunderstandings, that in the course of this paper I have purposely quoted the formulæ of preceding authors with the (older) symbols used by *them* to express vanadic acid. I do not intend to discuss the results of former analyses, because Professor Roscoe, in the course of his ingenious researches concerning vanadium, has expressly (see Proceedings of Royal Society, vol. xvi. p. 226) said, "It is the author's intention to investigate the composition of the vanadates at a future time."

could not detach any particle of the small group of crystals at my disposal for the purpose of chemical investigation, being thus obliged to confine it to two varieties of vanadinite from Kappel, and to a specimen of descloizite. On two specimens of vanadinite from Kappel, vanadinite appears in the form of crystals intimately connected into a crust, the single crystals not exceeding the size of $\frac{1}{2}$ – $\frac{3}{4}$ millim. On one specimen (variety A), the greater number of the larger crystals is of a dark greenish-brown colour (fluorescence-colour?), and without translucidity; the smaller ones are reddish brown and translucent; the streak is brownish orange. This variety agrees, in all its external characters, with the specimen of Peruvian descloizite now before me; the crystals of it are reddish to greenish brown, translucent, and their streak is brownish orange*.

The crystals of the second specimen (variety B) form a thin crust over limestone; they are $\frac{1}{2}$ –1 millim. in size, rather translucent, of lighter carnation tint, and less bright than those of variety A. The crystals of variety A show a vivid metallic brightness, while the light carnation ones of variety B scarcely offer a faint vitreous brightness, very easily altered by contact with moist air; even the moisture contained in the breath is sufficient to produce a superficial decomposition of the crystals of variety B. Their brightness is immediately tarnished, and in a few minutes very diminutive greyish-white globules cover, like *Mucedineæ*, the surface of the crystal. Some time after, these globules assume a slight yellowish colour, but spread no further over the specimen if it is kept in dry air. The supposition of this alteration being caused by the presence of arsenic is contradicted by the results of subsequent investigation; proving arsenic, if it exists at all, to exist in such diminutive proportion that visible marks of its presence could not be obtained by heating the mineral on charcoal or in a glass tube†. Notwithstanding the differences detailed above the actions of hydrochloric acid, of alcohol, and of bisulphate of potash give evidence of the notable difference between eosite, descloizite, and vanadinite, and of the nearly absolute identity of the two varieties of vanadinite from Kappel.

Small splinters of both these varieties, moistened with hydrochloric acid on a glass plate, become yellowish grey on their margins, the internal portion assuming a dark reddish-brown tint. Under the protracted action of the acid this nucleus becomes darker and lessens in size, till at last it disappears, leaving a portion of uniformly greyish-white substance. The nucleus of the carnation variety B shows a somewhat lighter tint at first; this difference, however, vanishes more and more under the lengthened action of acid; alcohol added to the cold solution of both varieties causes the separation of a yellowish-green gelatinous substance,

* See Schrauf "On the identity of vanadinite and descloizite" in Poggend. Ann. vol. cxvi. (1861).

† Several experiments intended for ascertaining directly the chemical character of these efflorescences remained without any satisfactory result.

which, being heated to evaporation, leaves on the glass plate a precipitate of fine green colour.

These alterations are common to both varieties, as also those produced by fusion with bisulphate of potash. The saline substance thus obtained is yellow during fusion; it becomes reddish during cooling, and deep reddish orange after complete refrigeration. This last coloration may exceed two or three times in intensity the coloration obtained by treating eosite with bisulphate of potash.

These experiments, merely tried with some few minute fragments, although any thing but decisive, are sufficient for ascertaining the difference between eosite and the vanadates of lead.

As the dark variety (A) resembles Peruvian eosite, the light variety (B) bears some resemblance to the dechenite from Schlettenbach.

This dechenite is likewise of carnation colour, somewhat fainter on the surface than on the recent fracture. The cabinet specimen before me shows isolated crusts, transversely concreted, with globular external surface, offering a blackberry-like aspect. A fracture shows these crusts to be concentric agglomerations of minute crystals (1 millim. in size), externally exhibiting only one or two of their planes. Isolated margins may be found by careful inquiry, but I could not succeed in obtaining any measurable angle. The crystalline crust includes a number of spheroidal cavities (similar to those of pisolite), around which the aggregations of crystallized dechenite are concentrically disposed. Greenish pyromorphite appears on the inferior surface of the specimen in question.

Like the variety B of vanadinite, this specimen is very easily affected by moisture. Even when a sheet of paper is held before the mouth, the moisture of the breath is sufficient to produce globular yellowish-grey efflorescences. The streak is orange, verging into reddish brown.

The action of hydrochloric acid and bisulphate of potash is nearly the same with that exercised on vanadinite, as already detailed.

A splinter moistened with hydrochloric acid shows a yellow margin and a darker nucleus, in which the vanadium is concentrated. The colour of this nucleus, instead of being very intensely brownish red, is of a rather darker brown than the unaltered mineral. The action of the acid, manifested by the progressive darkening and lessening of the nucleus, is, however, less prompt and less evident than when vanadinite is acted upon, and resembles rather the alterations undergone by eosite. The difference lies, however, in the yellowish coloration of the solution around the crystal lying on the glass plate. Alcohol gives rise to the secretion of a gelatinous yellowish-green liquid, which, evaporated by heat, assumes a fine green or bluish-green tint, nearly identical to the tint assumed by vanadinite under the same circumstances.

Dechenite, melted with bisulphate of potash, gives a deep brownish-yellow substance, reddening by cooling, and finally becoming of a deep

orange. This salt, heated in a platinum spoon with tin-foil, gives a faintly greyish-green solution.

The analogy between the vanadate of lead from Schlettenbach (dechenite) and the vanadate from Windisch-Kappel (vanadinite) goes still beyond their chemical characters. I submitted both these minerals to the action of the blowpipe, in order to find out, independently of my comparative studies concerning eosite, the cause of the prompt decomposition of the cabinet specimens from both these localities.

A splinter of dechenite from Schlettenbach, heated in a glass tube, emits, previously to its melting, a greenish-white vapour, not condensing into water drops nor into any other deposit upon the cooler portions of the tube. The presence of arsenic is manifested neither by its characteristic smell nor by any specular sublimate. The mineral takes a darker tint when heated, and assumes again its original tint by cooling. It melts in the glass tube, without previous decrepitation, into a brownish-yellow substance. Heated on charcoal, dechenite melts easily, and with ebullition, into a hollow dark steel-brown globule, giving at the same time a slight orange aureola and a whitish slag, including metallic granules. Fusing with soda gives rise to a yellowish slag, including granules of metallic lead, and to graphitic vanadium imbedded in the charcoal. The slag imbibed with cobalt-solution and heated to redness, assumes a dirty greenish tint and is surrounded by a greenish-yellow aureola, thus betraying the presence of a rather notable quantity of zinc, not mentioned in M. Bergemann's account of his analysis of dechenite.

Among the vanadinites from Kappel, only the carnation variety B seems to contain a notable proportion of zinc. Its tint darkens transitorily during heating, it emits a faint vapour (of water) when heated in a glass tube, without manifesting the presence of arsenic, and melts on charcoal into a dark greyish-brown, steel-bright globule, proving hollow when the flame of the blowpipe touches it. A notable aureola of oxidated lead appears, together with a small quantity of a dark slag, including globules of lead and graphitic vanadium, and assuming a green tint in contact with nitrate of cobalt.

The darker variety A exhibits the same phenomena when heated in the glass tube or by the blowpipe-flame, only the remaining slag is dark coloured, and only in one case among repeated experiments could I perceive on it some faint traces of green coloration. Possibly this variety may contain but a small proportion of zinc, as M. Damour has found only $2\frac{1}{2}$ per cent. of this metal in the Peruvian descloizite.

The results of the before-detailed investigations, leaving out of account at present the subsequent crystallographical facts, may be resumed as follows:—Of both the varieties of vanadinite from Kappel, the darker one (A) contains but a small proportion of zinc, and may be assimilated to the Peruvian descloizite; the lighter one (B) is far richer in this metal,

and may be identified with the dechenite from Schlettenbach. Eosite differs from both these minerals as to its chemical characters.

§ 5. Crystallographical form of Eosite.

Nearly all the eosite crystals attached to the surface of the cabinet specimen are octahedra, three-fourths of which are completely developed, only one of them showing one angle truncated by the basal plane c (0 0 1). The planes, instead of being completely even, are somewhat bent and scaly; however minute the planes p (1 1 1) and c (0 0 1) are, the intense metallic brightness of the crystals admits a precise determination of the angles.

The angles ascertained by measurement are :

Crystal I. $\{p(111)\}$.	Crystal II. $\{p(111)\}$.	Crystal III. $\{c(001); p(111)\}$.
$p : \bar{p} = 53^\circ 30'$	$p : 'p = 77^\circ 30'$	$c : p = 63^\circ 20'$
$p : 'p = 102^\circ 50'$	$'p : \bar{p} = 102^\circ 10'$	$c : 'p = 62^\circ 30'$
$'p : \bar{p} = 103^\circ$	$p : p' = 77^\circ 50'$	$c : p' = 62^\circ 50'$
$p : 'p = 77^\circ 50'$		$'p : \bar{p} = 103^\circ 25'$
		$p : \bar{p} = 53^\circ 50'$
		$p : p' = 77^\circ 30'$
		$p : 'p = 102^\circ$

The means of these results are:—

$$c : p = (001)(111) = 65^\circ 50'$$

$$p : 'p = (111)(\bar{1}11) = 77^\circ 50',$$

and consequently

$$(111) : (101) = 38^\circ 55'$$

$$(111) : (110) = 27^\circ 10'$$

$$(111) : (100) = 51^\circ 5'$$

$$(100) : (110) = 45^\circ 5'.$$

The preceding data lead to the parametrical proportion

$$a : b : c = 1.003 : 1 : 1.375,$$

not very far different from a pyramidal axial proportion. The last-quoted prismatic angle differs also only by $5'$ from the pyramidal principal prism. Notwithstanding the prisms with an angle of $90^\circ 10'$ being proper to several prismatic minerals, the slight difference obtained in the present case may be ascribed to an error of observation, owing to the exiguity and the curved surface of the planes, and eosite may be safely ranked among the pyramidal system, with the axial proportion

$$a : a : c = 1 : 1 : 1.3758, \quad cp = 62^\circ 50'.$$

This parametrical proportion of eosite, together with its chemical characters, proves its near relation to wulfenite and descloizite. Eosite unites

the crystallographical system and the lateral angles of wulfenite with the pyramidal angle of descloizite*,

Eosite having	62° 50',	descloizite	63° 35',	and wulfenite	65° 47'.
„	51° 5',	„	57° 35',	„	49° 50'.
„	51° 5',	„	44° 9',	„	49° 50'.

The proportion of the principal axis of eosite to the same axis of wulfenite is

$$\frac{1.375}{1.574} = \frac{7.0085}{8.0000}.$$

The pyramid of eosite could therefore be considered as a representative of the pyramid 778 of wulfenite, and the crystals of eosite to be merely a new form of the crystallographical form of the last-named mineral. Setting aside the chemical differences, a number of crystallographical characters may be alleged against the identity of these two minerals. All the crystals of eosite carefully examined by me show only one dominant pyramid, without any secondary pyramidal plane. The difference between the angles of the pyramids of eosite and wulfenite amounts to only 3°. It would have been too slight for admitting, with any degree of probability, that the secondary pyramid (778), having grown out into a principal plane, has completely superseded the original principal pyramid, had not an alteration in the chemical constitution of the substance been attended by an altered arrangement of the atoms entering into the composition of the crystalline molecule.

§ 6. *Crystallographical forms of Descloizite and Vanadinite.*

The similarity of the pyramidal angles of eosite and Peruvian descloizite (the second one, as stated by M. Des Cloizeaux) led me at first to suppose the existence of similar forms among the crystals of the carnation variety B of vanadite or of dechenite.

The crystalline agglomerations of dechenite are so confused that it was

* I alluded, a number of years ago, to the necessity of beginning any crystallogenetico theory with distributing the molecules of the elements constituting a combination according to the three directions of space, which thus is differentiated in the required way. In the case here in question the subsequently acceding substance (vanadium) seems to have assumed its direction according to the principal axis, the molecules of wulfenite having kept meanwhile their original direction along the secondary axis; and this supposition could, indeed, account for the successive transition of form from wulfenite into descloizite. However, such investigations could lead to any real, not merely apparent, success only under condition of not being confined to the parameter of the crystals, but also founded on the molecular values of the constituting elements.

These ideas of mine, and the calculations of crystalline forms from the molecular values of the elements founded on them, have been thoroughly discussed in several of my publications.

See Schrauf, *Physikalische Studien* (Vienna, 1867), c. xvi. p. 240; *Lehrbuch der Mineralogie*, vol. ii. *Krystall-Physik* (Vienna, 1868), c. xi. p. 160; *Pogg. Ann.* 1867, vol. cxxx. p. 33.

impossible to detach any complete crystal out of them. Here and there isolated lateral angles, some of them offering an angle of about 90° , are distinguishable.

As to the form of crystals, the variety B of vanadinite from Kappel bears some resemblance with these aggregations of dechenite. Some few solid angles of the variety B could be detached and their angles measured. The results thus obtained are:—

Crystal I.	Crystal II.	Crystal III.
$p : p' = 90^\circ$	$p : \bar{p} = 52^\circ$	$p : 'p = 64\frac{1}{2}^\circ$
		$p : p' = 91^\circ$
		$'p : p' = 127^\circ$.

These angles are perfectly concordant with those obtained by my former measurements (see Poggendorff's Ann. vol. cxvi.), as well as with those at present taken on the crystals of the dark variety A from Kappel. These last gave:

For crystal I.	For crystal II.	For crystal III. (with the planes p and m).
$p : 'p = 65^\circ$	$p : 'p = 64^\circ 50'$	$p : p' = 91^\circ 30'$
$p : p' = 92^\circ$	$'p : p' = 90^\circ$	$'p : 'p' = 90^\circ 50'$
$'p : p' = 128^\circ$	$'p' : p' = 63^\circ 30'$	$p : \bar{p} = 53^\circ 30'$
	$p' : p = 90^\circ$	$\bar{p} : 'p = 65^\circ 0'$
	$\bar{p}' : p = 116^\circ 10'$	$m : m' = 116^\circ 30'$
	$\bar{p} : 'p = 53^\circ$	$p : m = 32^\circ 50'$
	$p : 'p' = 127^\circ$	

All these numbers are in perfect concordance with those ascertained by M. Des Cloizeaux on the Peruvian descloizite; these are:—

$$\begin{aligned}
 m m' &= 116^\circ 25' \\
 m b\frac{1}{2} &= 147^\circ 45' \\
 b\frac{1}{2} b\frac{1}{2} &= 127^\circ 10' \\
 b\frac{1}{2} (m) b\frac{1}{2} &= 115^\circ 15' \\
 b\frac{1}{2} (e\frac{3}{2}) b\frac{1}{2} &= 88^\circ 18'.
 \end{aligned}$$

These results prove thus the absolute identity of the forms of Peruvian descloizite with those of both varieties of vanadinite from Kappel, probably also with those of dechenite, all of them discrepant from the characteristic forms of eosite. These investigations throw likewise some light on the indices of the planes observed on descloizite.

M. Des Cloizeaux has observed the three planes $m b\frac{1}{2}$ and $e\frac{3}{2}$, whose indices are, (1 1 0), (1 1 1), and (2 0 3). M. Des Cloizeaux expressly states that he could not find out from these data any isomorphism between this form and that of any other lead salt. I can, however, in no way explain how so eminent a mineralogist came to omit comparing the parameter of anglesite with that of descloizite, as he did with the parameter of cerussite.

Such a comparison proves immediately the isomorphism of both these minerals, the angles of anglesite being :

$$\begin{aligned} n n' &= 115^\circ 6' \\ a y &= 56^\circ 51' \\ b y &= 45^\circ 3' \\ c y &= 63^\circ 19', \end{aligned}$$

and those of descloizite :

$$\begin{aligned} m m' &= 116^\circ 25' \\ \frac{b\frac{1}{2} b\frac{1}{2}}{2} &= 57^\circ 35' \\ &= 44^\circ 9' \\ &= 63^\circ 35' \end{aligned}$$

M. Des Cloizeaux's symbols must consequently be admitted as equivalent* to

$$\begin{aligned} m' &= n (0 \ 2 \ 1) \\ b\frac{1}{2} &= y (2 \ 2 \ 1) \\ c\frac{3}{2} &= c (3 \ 0 \ 1). \end{aligned}$$

These notations and symbols I intend to adopt in future. This disposition equalizes the terminal angles of eosite with those of descloizite,—[*c y*] of descloizite being nearly equivalent to [*c p*] of eosite.

This isomorphism of anglesite (PbOSO_3) with descloizite ($2\text{PbO}, \text{VO}_3$, according to M. Damour) is only to be accounted for by granting to M. Damour's analysis less importance than its author's name allows to claim, and by admitting descloizite, vanadinite, and dechenite to be merely monovanadates. Thus another argument for separating these three minerals becomes untenable; the only distinctive character still remaining is the greater or lesser proportion of zinc contained in them.

§ 7. *Crystallographical form of the Chromo-Wulfenites.*

Prof. H. Rose, in his above-quoted investigation of the chromo-wulfenites, has given the measures of angles of the variety from Beresowsk, differing but slightly from the pyramidal angles generally admitted. I intend in the present paper to ascertain the angles of the chromo-wulfenites from Ruskberg and Phenixville, in order to state the changes arising from the accession of chromium to the constituting elements. The nature of the planes is, however, such that the measurements offer differences of nearly $\frac{1}{2}^\circ$, the averages from even a greater number of observations becoming thus rather objectionable.

* Concerning the indices and forms of anglesite, see my 'Atlas der Crystalformen des Mineralreiches,' under the article "Anglesite." The isomorphism of descloizite and anglesite appears in the similarity of forms, as well as in the equality of angles, the vanadinite from Kappel resembling the anglesite from Siegen, as the Peruvian descloizite is similar to the anglesite from Wolfach. See Schrauf's 'Atlas der Crystalformen,' No. 2 (Vienna, 1870), tab. xi. fig. 1, and tab. xii. fig. 32.

The crystals of chromo-wulfenite from Ruskberg are 1–2 millimetres in size. Most of them are quadrilateral pyramids, formed by the planes $e(1\ 0\ 1)$; in some of them the pyramid is truncated by the terminal plane $c(0\ 0\ 1)$. The planes are convex. My measurements gave:—

For crystal I.	For crystal II.
$c\ e = 57^{\circ}\ 30'$	$e\ \bar{e} = 65^{\circ}\ 10'$
	$e\ e' = 73^{\circ}\ 30'$

I did not succeed in finding in the crystals from Phenixville the form alleged by Prof. Dana (Mineralogy, 1868). Those before me exhibit the pyramid n , common to all wulfenites, assuming a tabular shape by the development of the truncating terminal plane $c(0\ 0\ 1)$. Their surface is opaque, rough, and curved, thus manifesting, as it were, the resistance opposed to regular development of the fundamental form, by the accession of heterogeneous substances. One of these crystals offered, besides the planes $c(0\ 0\ 1)$ and $n(1\ 1\ 1)$, two other planes, $m(1\ 1\ 0)$ and $f(3\ 2\ 0)$, the last of them in hemihedral development. I found on this last crystal:—

By measurement.	By calculation.
$n\ \bar{n} = 48^{\circ}\ 30'$	$n\ \bar{n} = 48^{\circ}\ 25'$
$n\ m = 24^{\circ}\ 20'$	$n\ m = 24^{\circ}\ 12\frac{1}{2}'$
$n\ f = 26^{\circ}\ 50'$	$n\ f = 26^{\circ}\ 35'$

As the results of measurements show, this crystallographical revision of chromo-wulfenites afforded no data for ascertaining the action of chromium on the molybdate of lead; they, however, confirmed the fact that the characteristic forms of eosite are wanting in the red varieties of wulfenite.

§ 8. *Discussion of Results.*

As a perusal of the last paragraph, treating of the morphological properties, will prove, these properties run parallel to the chemical characters of the minerals here in question. The results obtained may be concisely enounced thus:—

The crystalline form of eosite differs from those of descloizite and wulfenite, having the terminal lateral angles of descloizite united with the crystallographical system of wulfenite.

Eosite is a vanado-molybdate of lead.

The red varieties of wulfenite have an admixture of chromium, but are not crystallographically different from the other varieties.

Descloizite is isomorphous with anglesite; thus the results of the analysis of descloizite seem to be questionable.

The dark variety of vanadinite (A) from Kappel is identical with Peruvian descloizite; the light variety (B) from the same locality is nearly identical in chemical characters with the dechenite of Schlettenbach and, as to the form of its crystals, with descloizite.

These appear to me to be the results of the present investigations: they are still incomplete as to the crystalline forms of dechenite; I would, however, congratulate myself if the hints given in their exposition could provoke further search for distinctly crystallized groups of dechenite.

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- § 1. Paragenetic relations of eosite.
- § 2. Chemical characters of eosite.
- § 3. Chemical properties of the chromo-wulfenites.
- § 4. Chemical properties of the vanadates of lead (dechenite, descloizite, and vanadite).
- § 5. Crystallographical form of eosite.
- § 6. Crystallographical forms of descloizite and vanadinite.
- § 7. Crystallographical form of the chromo-wulfenites.
- § 8. Discussion of results.

May 11, 1871.

General Sir EDWARD SABINE, K.C.B., President, in the Chair.

The following communications were read:—

- I. "An Experimental Inquiry into the Constitution of Blood and the Nutrition of Muscular Tissue." By WILLIAM MARCET, M.D., F.R.S., Senior Assistant Physician to the Hospital for Consumption and Diseases of the Chest, Brompton. Received April 1, 1871.

(Abstract.)

The results obtained from the inquiry which forms the subject of the paper are as follows:—

First. That blood is strictly a colloid fluid.

Second. That although blood be strictly a colloid, it contains invariably a small proportion of diffusible constituents amounting to nearly 7·3 grms. in 1000 of blood, and 9·25 grms. in an equal volume of serum, these proportions diffusing out of blood in twenty-four hours.

Third. That the proportion of chlorine contained in blood has a remarkable degree of fixity, a fact which had been foreseen by Liebig, and may be considered as amounting to 3 parts (the correct mean being 3·06) in 1000, the proportion of chlorine in a bulk of serum equal to that occupied by 1000 grms. of blood being 3·45, and therefore higher than in blood, and moreover that one of the objects of the chlorides, and other diffusible constituents of blood, appears to be to preserve the fluid state of the blood. The substances which yield an alkaline reaction to blood are mostly crystalloid; their being retained in the blood while circulating through the body must be of the highest importance in connexion with the phenomena of oxidation constantly in progress during life.

Fourth. That blood contains phosphoric anhydride and iron in a perfect colloid state, or quite undiffusible when submitted to dialysis, the relative proportions appearing to vary from 78·61 per cent. of peroxide of iron and 21·39 of phosphoric anhydride, to 76·2 and 23·8 respectively, the proportion of phosphoric anhydride having a tendency to be rather higher.

Fifth. That blood contains more phosphoric anhydride and potash, bulk for bulk, than serum. This fact has long been known; but I have shown that the excess of phosphoric anhydride and potash in the blood-corpuscles is greater than can be accounted for by the estimation of the proportions of colloid phosphoric anhydride and potash in blood and serum; consequently there exists in blood-corpuscles a power checking the diffusion of the diffusible substances they contain, and apparently connected with a force peculiar to the corpuscles, as this force ceases to act as soon as the corpuscular form disappears from admixture with water. This property inherent to blood-corpuscles may cause an accumulation of potash in blood equal to no less than rather more than four times the amount of this substance present in an equal bulk of the serum of the same blood.

Sixth. That a mixture of colloid phosphoric anhydride and potash can be prepared artificially by the dialysis of a solution of chloride of potassium and phosphate of sodium, and that the colloid mass thus obtained appears to retain the characters of the neutral tribasic phosphate from which it originates; it exhibits an alkaline reaction, yields a yellow precipitate with nitrate of silver, and after complete precipitation the reaction is acid.

Seventh. That by dialyzing certain proportions of phosphate of sodium and chloride of potassium during a certain time, proportions of phosphoric anhydride, potash, chlorine, and soda are obtained in the colloid fluid very similar to the proportions these same substances bear to each other in serum after twenty-four hours dialysis.

Eighth. That muscular tissue is formed of three different classes of substances,—the first including those substances which constitute the tissue proper, or the portion of flesh insoluble in the preparation of the aqueous extract, and consisting of an *albuminous principle* and *phosphoric anhydride* with varying proportions of *potash* and *magnesia*; the second class including the same substances as are found in the tissue proper, and in the same proportions relatively to the albumen present in that class, but existing in solution and in the colloid state; the third class including the same substances as are found in the two others, and moreover a small quantity of chlorine and soda, which, although relatively minute, is never absent. The constituents of this class are crystalloid, and consequently diffusible, the phosphoric anhydride and potash being present precisely in the proportion required to form a neutral tribasic phosphate, or a pyrophosphate, as the formula 2KO PO_3 can equally be 2KO HO PO_3 . The formation of this substance (2KO PO_3) is extremely interesting, and shows beyond a doubt that, in addition to the material blood yields to muscu-

lar tissue for the formation of its insoluble portion, or framework, it supplies flesh with a large proportion of potash, the only object of which is the ultimate removal of the phosphoric anhydride it contains.

Class No. I. of the constituents of muscular tissue is to be considered as the tissue in the complete stage of assimilation.

Class No. II. constitutes the material from the blood on its way to form the Class No. I.

Class No. III. constitutes the material from Class No. I. in the effete state, and on its way out of flesh.

Ninth. That flesh contains in store a supply of nourishment equal to about one-third more than its requirement for immediate use, this being apparently a provision of nature to allow of muscular exercise during prolonged fasting.

Tenth. That the numbers representing the excess of phosphoric anhydride and potash in blood over the proportion of these substances in an equal volume of serum in the regular normal nutrition of herbivorous animals appear to bear to each other nearly the same relation as that which exists between the phosphoric anhydride and potash on their way out of muscular tissue, from which result I conclude that the blood-corpuscles have apparently the power of taking up and preparing the material which they themselves supply to muscular tissue for its nutrition.

Eleventh. That vegetables used as food for man and animals, such as flour, potato, and rice, contain respectively about the same proportions of colloid phosphoric anhydride and colloid potash compared to the total quantities of these substances present; this fact is very remarkable, considering that the proportions of phosphoric anhydride and potash vary to a great extent in these different articles of vegetable food. I also found in some of my analyses of blood that the proportions of colloid phosphoric anhydride and colloid potash to the total quantity of these substances was the same as the corresponding proportion existing between these substances in flour, potato, and rice; and I conclude that vegetable food has the power of transforming phosphoric anhydride and potash from the crystalloid or diffusible into the colloid or undiffusible state, and in certain tolerably definite proportions; and it is only after having been thus prepared that these substances appear to be fit to become normal constituents of blood, and contribute to the nutrition of flesh.

A final remark, and one which is worth consideration, is the fact established by the whole of the present investigation, that there is a constant change, as rotation in nature, from crystalloids to colloids and from colloids to crystalloids. The substances destined to nourish plants being inanimate must be diffusible, otherwise they could not be distributed throughout the mineral kingdom and brought within the reach of plants. Vegetables transform into colloids the mineral substances destined for the food of animals, and it might be said that the locomotion of animals in some respects acts the same part as the diffusion of mineral substances; for animals

move about in search of their colloid food, and crystalloid minerals are displaced by physical diffusion in search of the plants they are to nourish.

The excretory products of animals are crystalloid and diffusible, as far as these soluble constituents are concerned, the solid portions rapidly decomposing in contact with air and moisture into crystalloid compounds. Dead vegetable and animal tissue all return into crystalloids by decomposition, to be distributed afresh, either by gaseous or liquid diffusion, throughout the whole of the mineral world.

Hence Graham's great discovery of the laws of liquid and gaseous diffusion lifts up the curtain which veils the mysteries of animal life, and throws a flood of light on very many physiological phenomena which had until now remained in darkness*.

II. "On Protoplasmic Life." By F. CRACE-CALVERT, F.R.S.

Received May 8, 1871.

A year since, the publication of Dr. Tyndall's interesting paper on the abundance of germ-life in the atmosphere, and the difficulty of destroying this life, as well as other papers published by eminent men of science, suggested the inquiry if the germs existing or produced in a liquid in a state of fermentation or of putrefaction could be conveyed to a liquid susceptible of entering into these states; and although at the present time the results of this inquiry are not sufficiently complete for publication, still I have observed some facts arising out of the subject of protoplasmic life which I wish now to lay before the Royal Society.

Although prepared, by the perusal of the papers of many workers in this field, to experience difficulties in prosecuting the study, I must confess I did not calculate on encountering so many as I met, and especially those arising from the rapid development of germ-life, and of which I have hitherto seen no notice in any papers which have come under my observation. Thus, if the white of a new-laid egg be mixed with water (free from life), and exposed to the atmosphere for only fifteen minutes, in the months of August or September, it will show life in abundance. From this cause I was misled in many of my earlier experiments, not having been sufficiently careful to avoid even momentary exposure of the fluids to the atmosphere. To the want of the knowledge of this fact may be traced the erroneous conclusions arrived at by several gentlemen who had devoted their attention to the subject of spontaneous generation.

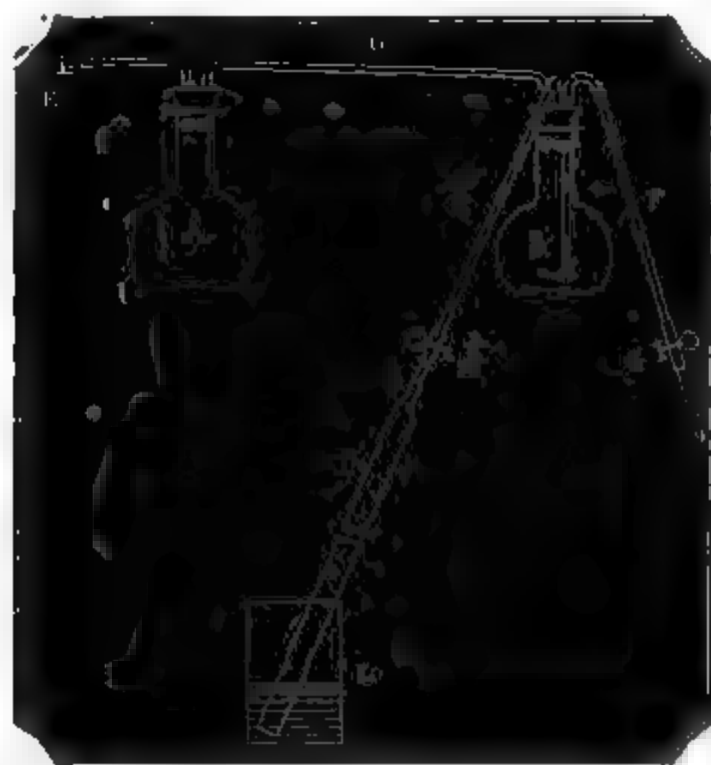
I believe that I have overcome the difficulty of the fluids under examination becoming polluted by impregnation by the protoplasmic life existing in the atmosphere, by adopting the following simple method of working.

As a pure fluid free from life, and having no chemical reaction, was essential to carrying out the investigation, I directed my attention to the

* I have had the valuable assistance of Messrs. M. T. Salter, F. A. Manning, and H. Bassett in the analytical part of my inquiry.

preparation of pure distilled water. Having always found life in distilled water prepared by the ordinary methods, by keeping it a few days, after many trials I employed the following apparatus, which gave very satisfactory results, as it enabled me to obtain water which remained free from life for several months.

It consists of two flasks, A and B (A rather larger than B), fitted with perforated caoutchouc stoppers*. These flasks are connected by the tube D. Into the stopper of A is fitted a tube C, to which is joined a piece of



caoutchouc tubing, which may be closed by the clip E. Through the stopper of B is a siphon, F, the long limb of which is cut and joined with caoutchouc tubing, which can be closed by the clip G. Through this stopper is a third tube, H, connected by caoutchouc with the tube I; this can be closed by the clip K. The tube I is about 3 feet long, and goes into the vessel L, which is partly filled with water.

The water to be distilled is mixed with solution of potash and permanganate of potash, and placed in the flask A†. Before distillation is commenced, a rapid current of pure hydrogen, or some other gas, must be passed through the apparatus by the tube C to displace the air and carry off all the germs the air may have contained. The clip G is first left open, then this closed and the clip K opened, which allows the gas to pass through the water in the vessel L.

* The stoppers and caoutchouc tubing used for the various joints must be new, and must be well boiled in water before use.

† The reasons why I employed permanganate of potash (in large excess) were that, under the influence of heat, its oxidizing powers were much increased, and that it gave off no gas that could interfere with the purity of the water, this salt in solution not even yielding oxygen under any circumstances.

The gas should be passed through for about fifteen minutes. The clip E is then closed, and the distillation carried on. When the operation is complete, the gas must be again passed through the apparatus, and the connexion with the tube I broken by closing the clip K. The water is drawn off through the siphon F. The long tube acts as a safety-tube, and is made so long that the absorption is noticed in ample time to close the clip before any air can enter through that tube.

The water has to be redistilled three or four times before it is obtained free from germs, and must be kept in the apparatus in which it is distilled until wanted, to prevent any contact with air.

Some water which had been distilled on the 20th of November, 1870, being still free from life on the 7th of December, was introduced by the siphon H into twelve small tubes, and left exposed to the atmosphere for fifteen hours, when the tubes were closed. Every eight days some of the tubes were opened, and their contents examined. On the fifteenth, therefore, the first examination was made, when no life was observed; on the twenty-third two or three other tubes were examined, and again no life was detected; whilst in the series opened on the 2nd of January, 1871 (that is to say, twenty-four days from the time the tubes were closed), two or three black vibrios were found in each field.

Being impressed with the idea that this slow and limited development of protoplasmic life might be attributed to the small amount of life existing in the atmosphere at this period of the year*, a second series of experiments was commenced on the 4th of January. The distilled water in the flask being still free from life, a certain quantity of it was put into twelve small tubes, which were placed near putrid meat at a temperature of 21° to 26° C. for two hours, and then sealed. On the 10th of the same month the contents of some of the tubes were examined, when two or three small black vibrios were observed under each field. This result shows that the fluid having been placed near a source of protoplasmic life, germs had introduced themselves in two hours in sufficient quantity for life to become visible in six days instead of twenty-four. Other tubes of this series were opened on the 17th of January, when a slight increase of life was noticed; but no further development appeared to take place after this date, as some examined on the 10th of March did not contain more life than those of the 17th of January.

This very limited amount of life suggested the idea that it might be due to the employment of perfectly pure water, and that the vibrios did not increase from want of the elements necessary for sustaining their life. I therefore commenced a third series of experiments. Before proceeding to describe this series, I would call attention to the fact that the water in the

* During the intense cold of December and January last I found it took an exposure to the atmosphere of two days at a temperature of 12° C. before life appeared in solution of white of egg in the pure distilled water, whilst as the weather got warmer the time required became less.

flask had remained perfectly free from life up to this time, a period of close on sixteen weeks.

On the 9th of February 100 fluid grains of albumen from a new-laid egg were introduced, as quickly as possible and with the greatest care, into 10 ounces of pure distilled water contained in the flask in which it had been condensed, and an atmosphere of hydrogen kept over it. On the 16th some of the fluid was taken out by means of the siphon H, and examined, and no life being present, twelve tubes were filled with the fluid, exposed to the air for eight hours, and closed. On the 21st the contents of some of the tubes were examined, when a few vibrios and microzyma were distinctly seen in each field. On the 27th other tubes were examined, and showed a marked increase in the amount of life. In this series life appeared in five days, and an increase in ten, instead of requiring twenty-four days, as was the case when pure water only was employed.

Albumen therefore facilitated the development of life. Of course the contents of the flask were examined at the same time, but in no instance was life detected. I believe that these three series of experiments tend to prove the fallacy of the theory of spontaneous generation; for if it were possible, why should not life have appeared in the pure distilled water, or in the albuminous solution, which were kept successively in the flask B, as well as in the fluids which were contained in the tubes, and had been exposed to the atmosphere or near animal matter in a state of decay, and had thus become impregnated with the germs of protoplasmic life? What gives still further interest to these experiments is, that, having operated during the severe weather of last winter, when little or no life existed in the atmosphere, I was able to impregnate the fluids with germs without introducing developed life.

The quantity of life produced in the above-recited experiments being comparatively small, I was led to infer that this might be due to the influence of the atmosphere of hydrogen employed to displace the air in the apparatus used for obtaining the water. I therefore, on the 2nd of March, prepared a solution of albumen similar to that before employed, but expelled the air out of the apparatus by pure oxygen; and as the contents of the flask B were free from life on the 8th of March, a series of small tubes were filled and exposed for twenty-six hours to the atmosphere near putrid matter, and then sealed. Several of these tubes were opened on the 11th, and immediately examined, when only a few cells were observed in each field. A second lot was opened on the 14th, and they showed considerable increase of life, there being two or three vibrios under each field. A third quantity was opened on the 25th, when no increase had taken place. This latter result tends to show that although oxygen appears to favour the development of germs, still it does not appear to favour their reproduction.

As the weather had become much warmer, and a marked increase of life in the atmosphere had taken place, some of the same albumen solution as

had been employed in the above experiments was left exposed in similar tubes to its influence, when a large quantity of life was rapidly developed and continued to increase. This result appears to show that the increase of life is not due to reproduction merely, but to the introduction of fresh germs; for, excepting this fresh supply, there appears to be no reason why life should increase more rapidly in the open than in the closed tubes.

In concluding this paper I have great pleasure in recognizing the able and persevering attention with which my assistant, Mr. William Thompson, has carried out these experiments.

III. "Action of Heat on Protoplasmic Life." By F. CRACE-CALVERT, F.R.S. Received May 9, 1871.

Those investigators of germ-life who favour the theory of spontaneous generation have assumed that a temperature of 212° Fahr., or the boiling-point of the fluid which they experimented upon, was sufficient to destroy all protoplasmic life, and that the life they subsequently observed in these fluids was developed from non-living matter.

I therefore made several series of experiments, in the hope that they might throw some light on the subject.

The first series was made with a sugar solution, the second with an infusion of hay, the third with solution of gelatine, and the fourth with water that had been in contact with putrid meat. The hay and putrid-meat solutions were taken because they had often been used by other investigators; sugar was employed, being a well-defined organic compound free from nitrogen, which can easily be obtained in a state of purity; and gelatine was used as a nitrogenized body which can be obtained pure and is not coagulated by heat.

To carry out the experiments I prepared a series of small tubes made of very thick and well-annealed glass, each tube about four centimetres in length, and having a bore of five millimetres. The fluid to be operated upon was introduced into them, and left exposed to the atmosphere for sufficient length of time for germ-life to be largely developed. Each tube was then hermetically sealed and wrapped in wire gauze, to prevent any accident to the operator in case of the bursting of any of the tubes. They were then placed in an oil-bath, and gradually heated to the required temperature, at which they were maintained for half an hour.

Sugar Solution.—A solution of sugar was prepared by dissolving 1 part of sugar in 10 parts of water. This solution was made with common water, and exposed all night to the atmosphere, so that life might impregnate it. The fluid was prepared on the 1st of November, 1870, introduced into tubes on the 2nd, and allowed to remain five days. On the 7th of November twelve tubes were kept without being heated, twelve were heated to 200° Fahr., twelve to 300°, and twelve to 400° Fahr.

The contents of the tubes were microscopically examined on the 1st of December, twenty-four days after heating.

Sugar solution not heated.	Heated for half an hour at 212° Fahr.	Heated for half an hour at 300° Fahr.	Heated for half an hour at 400° Fahr.	Heated for half an hour at 500° Fahr.
There were about 30 animalcules under each field of the microscope, principally <i>small black vibrios</i> , 2 or 3 microzymes swimming slowly about, 3 or 4 <i>ordinary swimming vibrios</i> , and a few <i>Bacteria</i> .	A great portion of the life had disappeared, no animalcules were swimming; still this temperature had not completely destroyed life. 4 or 5 <i>small black vibrios</i> were observed moving energetically to and fro; 2 or 3 <i>ordinary vibrios</i> were also observed moving energetically in the same position of the field, that is, without swimming about.	The sugar was slightly charred, but the life was not entirely destroyed, as 1 or 2 <i>ordinary vibrios</i> and 1 or 2 <i>small black vibrios</i> were observed in motion under the field of the microscope.	The sugar was almost entirely decomposed; no trace of life was observed.	No life observed.

Remarks.—The black vibrios here referred to are far more opaque than the other varieties of vibrios, and are the most important of all, as I have found them to resist not only very high temperatures, but all chemical solutions. I shall, in my paper on putrefaction and the action of anti-septics, describe the various vibrios and give drawings of them.

Hay Infusion.—An infusion of hay was made by macerating it in common water for one hour, then filtering the liquor, and leaving it exposed to the atmosphere all night, when it was sealed in the small tubes, twelve of which were used for each experiment. The infusion was made on the 4th of November, sealed in tubes on the 5th, and heated on the 7th.

The results were examined on the 1st of December, 1870, twenty-four days after being heated.

Hay infusion not heated.	Heated for half an hour at 212° Fahr.	Heated for half an hour at 300° Fahr.	Heated for half an hour at 400° Fahr.	Heated for half an hour at 500° Fahr.
Fungus matter was observed growing on the surface of the fluids in two of the tubes. On subjecting the contents of some of the tubes to examination, from 20 to 25 animalcules were observed under each field of the microscope. This kind of life resembled small dots moving energetically to and fro; 1 or 2 ordinary vibrios were also present.	No fungus matter was noticed on the surface in any of the tubes. A few small black vibrios present in the original solution were also present in this.	No fungus matter present, but some of the small black vibrios were still present, although in less numbers.	No fungus matter observed. The fluid was filled with irregular masses of coagulated matter, and life had disappeared.	No life present.

Gelatine Solution.—A solution of gelatine, prepared of such strength that it remained liquid on cooling, was exposed for twenty-four hours to the atmosphere. It was then introduced into the small tubes, and the tubes sealed. The solution was made on the 4th of November, the tubes sealed on the 5th, and subjected to the different temperatures on the 7th.

The fluids were examined on the 1st of December, 1870, twenty-four days after being heated.

Gelatine solution not heated.	Gelatine solution heated for half an hour at 100° Fahr.	Heated for half an hour at 212° Fahr.	Heated for half an hour at 300° Fahr.	Heated for half an hour at 400° Fahr.
There were 7 or 8 animalcules under each field, 5 or 6 of which were quite different to any thing observed in the other fluids. They had long thin bodies, swimming with a peristaltic motion. 1 or 2 ordinary swimming vibrios were also present; but the small black vibrios were absent.	Life seemed to have only slightly decreased, and none of the animalcules were swimming. The peculiar animalcule mentioned in the first column appeared to retain still its peristaltic motion, but not sufficient power to move across the field, a few ordinary vibrios being also observed moving to and fro.	A very decided diminution in the quantity of life present was noticeable.	No life present.	No life present.

Putrid-Meat Fluid.—Water was placed in an open vessel, and a piece of meat suspended in it until it became putrid and contaminated with myriads of animalcules. This fluid was placed in the usual tubes, which were sealed on the 7th of November, and heated on the same day.

The contents of the tubes were subjected to examination on the 1st of December, or twenty-four days after having been heated.

Not heated.	Heated for half an hour at 100° F.	Heated for half an hour at 212° F.	Heated for half an hour at 300° F.	Heated for half an hour at 400° F.	Heated for half an hour at 500° F.
A large quantity of life was present, namely, microzyma and several distinct species of vibrios, among which were a number of the small black ones frequently mentioned.	This temperature had but slightly affected the life present, the animalcules being as numerous as in the liquid not heated, and moving as usual. However, one species of very long vibrios appeared to be considerably affected, as they were much more languid in their movements.	This liquor differed from all the others in being turbid and coagulated. Life was still present; and although heat had deprived the animalcules of the power of locomotion, still they retained a sufficient amount of vital force to place it beyond a doubt that life was not destroyed.	The liquid was quite clear, the albumen (which is coagulated at 200°) appearing to be redissolved. A large quantity of the life in the fluid was destroyed, but some vibrios still remained, the small black ones being the most numerous.	All life had disappeared.	All life had disappeared.

The results recorded in the above Tables show that protoplasmic life is but slightly affected by a temperature of 212° F., and that, even at a temperature of 300° F., it is not entirely destroyed, excepting in the case of gelatine. In all the other fluids a temperature of 400° F. is necessary to completely destroy the life. These experiments, therefore, clearly show that the life found by previous experimenters in fluids which have been submitted to heat was not due to heterogenesis, but to life which had remained in the fluids, as I have seen no experiment reported where the temperature to which the fluids were exposed exceeded 300° F.*

I am the more justified in making this statement, as I have repeatedly examined the contents of tubes which had been submitted to a temperature of 400° F., both immediately after cooling and at all periods up to thirty days, and was unable in any instance to detect the slightest trace of life.

This important result corroborates those recorded in my previous paper, and proves that the spontaneous-generation theory is not yet by any means established.

* It is with pleasure that I find these experiments to confirm the suggestion of Dr. Beale, in his work entitled "Disease-Germs, their supposed Origin," page 50 (which I read a few weeks ago), that "living forms might live though exposed, under certain conditions, to a temperature of 350° F."

It occurred to me that it might be interesting to examine the influence on pure albumen of the putrid-meat fluids that had been heated, and note whether they still possessed the property of propagating life. A solution was prepared by mixing the albumen of a new-laid egg with pure distilled water free from life (prepared as described in my previous paper). Equal volumes of this solution were placed in six small test-tubes, which had been cleansed with hot vitriol and well washed with pure water. To one tube two drops were added of the putrid-meat solution that had been heated to 100° F., to a second two drops of that heated to 212° F., to a third two drops of that heated to 300° F., to a fourth an equal bulk of fluid heated to 400° F., and to a fifth the same quantity heated to 500° F. In the sixth the albuminous solution, without any thing added, was kept for comparison.

The tubes were sealed, and kept from the 1st of February to the 9th.

RESULTS OF EXAMINATION.

Albumen solution.	Albumen solution, with putrid-meat liquor, heated to 100° F.	Albumen solution, with putrid-meat liquor, heated to 212° F.	Albumen solution, with putrid-meat liquor, heated to 300° F.	Albumen solution, with putrid-meat liquor, heated to 400° F.	Albumen solution, with putrid-meat liquor, heated to 500° F.
In each drop 2 or 3 small black vibrios, moving to and fro.	Abundance of life.	Abundance of life.	Much less life than in the two fluids previously examined.	In each drop 2 or 3 small black vibrios, moving to and fro.	In each drop 2 or 3 small black vibrios, moving to and fro.

These results clearly show that, at the temperatures of 100°, 212°, and 300° F., life and its germs had not been destroyed, whilst at 400° F. they had; for the results of the examination were in this case exactly identical with those of the albumen solution itself; and the life found was doubtless introduced in the preparation of the solution, and was not due to any life having remained in the fluids that had been heated.

Although perfectly aware of the interesting researches of Professor Melsens, proving that the most intense cold does not destroy the active power of vaccine lymph, still I thought it desirable to ascertain the effect of a temperature of 15° F. on well-developed germ-life, similar to that which had been subjected to the action of heat.

Some putrid-meat liquor, therefore, containing a large quantity of microzyma and vibrios, was subjected for twenty hours to the influence of a temperature ranging between the freezing-point of water and 17° below that point, when the ice was melted and the liquor examined. The animalcules retained their vitality, but appeared very languid, and their power of locomotion was greatly decreased.

Two hours after melting the ice the liquor was again examined, when the animalcules appeared to be as energetic as before.

The Society adjourned over Ascension Day, to Thursday, May 25th.

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May 25, 1871.

General Sir EDWARD SABINE, K.C.B., President, in the Chair.

Pursuant to notice given at the last Meeting, Capt. Douglas Galton proposed and Prof. Huxley seconded the Right Hon. Robert Lowe, M.P., for election and immediate ballot.

The ballot having been taken, Mr. Lowe was declared duly elected.

The following communications were read:—

- I. "On the Temperature of the Interior of the Earth, as indicated by Observations made during the Construction of the Great Tunnel through the Alps." By D. T. ANSTED, M.A., F.R.S., For. Sec. G.S. Received April 6, 1871.

It had been arranged from the commencement of the Alpine tunnel (often, though incorrectly, called the Mont-Cenis tunnel) that observations should be taken at intervals of about one kilometre (3281 feet) from both the French and Italian ends, with a view to determine as nearly as possible the law of increment; and before the completion of the work it had become evident that the ordinary estimate of increased temperature due to the depth below the surface of the earth would by no means apply to this particular case. The actual length of the tunnel being 40,140 feet, and the culminating point of the mountain being 5280 feet vertically above a point 21,156 feet from the Italian end, there was evidently scope for a number of valuable observations. The result also might be expected to show the influence of marked irregularities in the contour of the mountains, and also of changes in the nature of the rock. The experiments and observations were entrusted to the resident engineers at each end of the tunnel, Signor Borelli undertaking them from the Italian side. It does not appear that any special construction of thermometer was supplied, or that the subject greatly interested the resident engineers. Careful observations were made and duly recorded by Signor Borelli, but the attempt failed in the hands of his colleague at the other end. Thus one great source of value was lost, and the observations at equal distances from both ends cannot be compared.

The rocks through which the tunnel has been driven consist to a very large extent of a peculiar calcareous schist, partly talcose, and containing many bands and strings of quartz. The whole of the Italian end is through rock of this kind, and it was reached at about 11,000 feet from the entrance from the French side. The rocks on the French side were at first very different, including nearly 7000 feet of a peculiar sandstone with indications of anthracite, about 1000 feet of very hard quartzite, and 3000 feet of gypsum, limestone, and calcareous schist, a large proportion being gypsum. It would have been in the highest degree desirable to have been able to compare observations of temperature made at similar depths in different

kinds of rock. All the rocks may be regarded as metamorphic; they are, however, stratified, dipping at an angle of 50° , or thereabouts, to the north-west, and corresponding in age to the secondary rocks of England, from the Oxford clay to the Rhætic inclusive. There was very little water met with in tunnelling.

The observations made by Signor Borelli have been recently made public in a memoir by Signor F. Giordano, that appeared in the *Bulletin of the 'R. Comitato Geologico d'Italia'* (No. 1, 2, Jan. and Feb. 1871); and in this communication the general subject is discussed. The following account, though not a translation of M. Giordano's memoir, derives most of its facts from the statements there made. The geological notes and some of the conclusions, however, result from the author's personal observations, assisted by the account published last year by Professor Sismonda, and a memoir that appeared also last year in the '*Comptes Rendus*,' written by M. Elie de Beaumont.

The temperature observations include the temperature of the air, of the water issuing in springs (always small) met with in the progress of the works, and of the rock. To obtain the latter, borings were made either in the walls of the tunnel or in the headings in advance of the tunnel, and generally to a distance of 7 to 10 feet. The excavation of the complete tunnel from the Italian end was suspended when 6 kilometres had been completed (about 20,000 feet, or less than halfway); but as the excavation from this end had been much more rapid than from the other, the work was continued by a small heading to a further distance of about 3000 feet, when the opening was made to the work on the French side. Thus several of the observations were made by borings into the wall of the heading, and at a long distance from the completed tunnel and from good ventilation.

No observations whatever are recorded from the French side of the tunnel, either during the work or at the time of meeting. We are informed, however, that a rush of air took place at the moment of the last blast, driving the smoke rapidly towards the Italian end. It should be mentioned that the northern, or French end, is only 1160^m (3806 feet) above the sea, whereas the Italian end is 1292^m·50 (4241 feet), showing a difference of level of 435 feet. Thus the tunnel would seem to act as a chimney, and it is not unlikely that a natural ventilating-current may be established.

On the morning of the 26th of December, 1870, the day when the communication was made, the external temperature in the Bardonneche valley was considerably below the freezing-point, but within the mouth of the tunnel several degrees above. On this day, besides the temperature of the rock, that of the small springs near the end and that of the air were recorded; but the number of men employed, the frequent blasts, and the active works going on must have had a marked influence on the latter, especially as for 3300 feet the excavation of the tunnel had been stopped and only a heading pushed on in advance. During the continuance of the

work, the smoke and foul air were drawn out of the tunnel by a ventilator near the roof.

The following tabular statement, adapted from that published by Signor Giordano, will show the nature of the observations for temperature made in the course of the works from the Italian side:—

No. of observations.	Distance in feet from S. entrance.	Temperature.		Observations.	Depth in feet.
		Air.	Rock or water.		
1.	1312	° F. 50·9	° F. 51·8	Small spring.	
2.	1640	50·9	57·6	Boring from heading 24 feet from wall of tunnel	1500
3.	3281	59·5	62·6	Boring of 16 feet from heading 28' from wall	1700
4.	3675	"	62·6	Small spring	
5.	6562	64·0	67·0	Boring of 10' from heading. 21½' from wall	
6.	8202	"	68·0	Small spring	
7.	9266	"	"	Ditto	3000
8.	9843	68·5	73·0	Boring of that like No. 5	
9.	13124	73·4	74·5	Ditto	
10.	16404	76·1	81·5	Ditto	
11.	19686	80·2	84·0	Boring of 10' in a recess 13' from wall near the point where the excavation of the tunnel was suspended till the communication was opened with the French side.	4500
12.	21156	86·2	85·1	Boring of 7' under the culminating point of the mountain with 5280 feet of rock overhead. Small heading 7' from wall.....	5280
13.	21858	"	82·4	Small spring.	4750
14.	22967	77·0	80·6	Boring of 7' into wall of small heading	
15.	22993	"	77·9	Small spring. Initial temperature probably rather higher.	

It thus appears that the observed difference of temperature of the rock between the distance of 1640 feet from the entrance and the distance of 21,156 feet is 27°·5 F. The difference of depth beneath the surface in that distance is about 4600 feet. It is thought possible that the real difference may be somewhat less, as the increased temperature of the air in the heading, owing to the number of men employed and the frequent blasting, may have influenced the result in some measure ; and it is perhaps safer to estimate the total increment at something less than 26½° F.

In the absence of observations that can be absolutely depended on, we may perhaps assume the true maximum temperature of the rock to be 84°. The part of the tunnel having this permanent temperature is 1295^m or 4250 feet above the sea, and the corresponding point of the surface is 9530 feet above the sea, showing a difference of 5280 feet. A careful esti-

mate of the distribution of the mountain mass seems to show that this is somewhat in excess of the true difference, and that if the slope were perfectly even the difference would be reduced to about 5080 feet.

It is necessary now to estimate as nearly as possible the mean annual temperature of the air at the surface, and the depth and temperature of the stratum of invariable temperature within the earth. None of these has as yet been determined by experiment; but it is found generally that the stratum of invariable temperature is nearly 2° F. warmer than the mean temperature of the air at the surface, and that the mean temperature of the air decreases in ascending to the higher parts of the atmosphere at the rate of 1° F. in 317 feet. As, however, the mean temperature of the air at the mouth of the tunnel is not known by observation, we must take Turin as the nearest point of departure, this city being 820 feet above the sea, and its mean annual temperature $54^{\circ}\cdot 5$ F. The difference of level between Turin and the mountain-summit being 8710 feet, this, divided by 317, gives $27^{\circ}\cdot 5$ as the amount to be deducted from $54^{\circ}\cdot 5$. Thus the calculated mean annual temperature would be $27^{\circ}\cdot 5$ F.; adding to this 2° , we have the calculated temperature of the stratum of invariable temperature $29^{\circ}\cdot 5$ F.

As some check on this estimate, it may be worth while to refer to a somewhat analogous case determined by observation by Dolfuss-Ausset, in 1865-66, on the mountain of St. Theodule and in the valleys of Aosta and the Vallais. In this case the mean temperature of the air was found to be $-5^{\circ}\cdot 10$ C. ($22^{\circ}\cdot 8$ F.) at the height of 3333^m (10,936 feet). The excess of elevation of this summit above the crest of the Alps over the tunnel being 1406 feet, the latter should be (at the rate of 1° F. per 317 feet) $4^{\circ}\cdot 5$ F. warmer, or have a temperature of $27^{\circ}\cdot 3$ F., a result nearly in accordance with the other calculation.

Estimated in this way, the difference of temperature between the mean temperature of the air on the assumed surface above the central point of the tunnel would be ($84^{\circ} - 27^{\circ}\cdot 5 =$) $56^{\circ}\cdot 5$ F., and the rate of increment (the difference of level being 5080 feet) 1° in $\left(\frac{5080}{56\cdot 5} =\right)$ 90 feet nearly; or if we assume the stratum of invariable temperature to be 80 feet below the surface, the rate will be 1° in $\left(\frac{5000}{54\cdot 5} =\right)$ 91 feet, showing, no doubt, a very considerable difference when compared with most other observations made in Europe and elsewhere at various levels, but not altogether unparalleled in special cases. It is, of course, possible that the difference may be due to the topographical conditions and geological structure of the earth's crust under the crest of a great mountain-chain of comparatively recent elevation.

But there is an important fact to be observed not alluded to by Signor Giordano, but bearing very strongly on the general question of the rate of increment. The slope of the Alps in this part of the range above the tunnel is nearly regular on the French side, but very sudden at first towards Italy, after which there is a wide step or terrace, for a distance of

about two miles. It results that the depth from the summit of the ridge to the tunnel being 5280 feet, the depth from the surface to the tunnel at the Torrent of Merdovine, where the step or terrace commences, and which is 8240 feet in horizontal distance from the summit, is only 1700 feet, showing a diminution of 3580 feet of elevation in 8240 feet of distance, or, allowing for a sudden rise near the summit, a slope of 1 in $2\frac{1}{2}$, or an angle of 21° . On the other hand, the depth from the surface to the tunnel between the Torrent of Merdovine and a point about 3000 feet from the entrance (a horizontal distance of 10,000 feet) shows hardly any appreciable difference, varying only a few hundred feet from one point in the profile to another. Beyond this point there is another rapid descent to the valley of Bardonneche, where the mouth of the tunnel is situated.

Temperature observations were taken (1) at a point under the summit of the crest, (2) at a point under the Torrent of Merdovine, and (3) under the last point mentioned, 3000 feet from the entrance. These show respectively $85^\circ\cdot1$ F., $74^\circ\cdot5$ F., and $62^\circ\cdot6$ F. In each case the observations were made from borings, and they may all be regarded as good and fairly to be compared with each other. Making the corrections already explained, the result would be, first, that borings of 1700 feet made in different parts of the terrace below the steep mountain face for a breadth of about two miles might show a rate varying from 1° of increase of temperature in 43 feet of depth to 1° in 63 feet; whilst on the steep slope the rate at the lowest part of the slope being 1° in 63 feet, the rate at the summit is 1° in 91 feet, the horizontal distance between the two places of observation being 8240 feet, or about a mile and a half. In all these cases there is no difference in the nature of the rock.

There are two intermediate observations, numbered (10) and (11) in the Table, which correspond to depths below the surface of 3000 and 4500 feet respectively. Estimated as before, the rate of increment is in the former case 1° in 65 feet, in the latter 1° in 84 feet.

One observation (No. 14) was made 1800 feet beyond the centre of the tunnel towards the north, at a depth which may be fairly estimated at 4750 feet. The temperature observed was $80^\circ\cdot6$ F., which, reduced as before, shows a rate equivalent to 1° in 934 feet.

These results tabulated will appear as follows:—

	Distance from S. entrance.	Depth from surface.	Temp. °	Rate of increment.
Observation No. 3 ..	3281	1700	$62^\circ\cdot6$ F.	1° F. in 43 feet.
5 ..	6562	„	$67^\circ\cdot0$	„ 50 „
8 ..	9843	„	$73^\circ\cdot0$	„ 61 „
9 ..	13124	„	$74^\circ\cdot5$	„ 63 „
10 ..	16405	3000	$81^\circ\cdot5$	„ 65 „
11 ..	19686	4500	$84^\circ\cdot0$	„ 84 „
12 ..	21156	5280	$85^\circ\cdot1$	„ 91 „
	From N. end.			
14 ..	18345	4750	$80^\circ\cdot6$	„ 934 „

The absence of observations from the French side appears more and more unfortunate as we advance in the investigation, and renders the general result by no means so satisfactory as it would otherwise be. It would appear, indeed, that distance from the surface alone is not by any means the only cause of increased temperature in the case of a lofty mountain mass; but how far this is due to imperfect observation, the want of properly sheltered instruments, and local causes connected with the progress of the works, it is not easy to say. There may also, no doubt, be some difference arising from the imperfect modes of estimating the mean annual temperature and the temperature and depth of the stratum of permanent temperature. But although all these are subject to a certain amount of correction, there is enough general accordance of the observations to show that the conclusions indicated must be accepted. Considering that we are in the heart of the great mountain axis of Europe, the conditions are simple and favourable. There is little or no permanent snow or ice on either side, and no glaciers: one slope (that to the south) is at first rapid, and then in steps; the other slope is very regular. The tunnel is of great length, and most parts of it of enormous depths below the surface, compared with any other depths that have been reached; and it has been mentioned already that the geological conditions are unusually simple, especially in the southern end of the tunnel, to which all the observations are confined. No case has ever before occurred in which there was so much opportunity of making systematic and trustworthy observations on the subject of the internal temperature of the earth; but as it is not unlikely that the successful completion of the tunnel under the Mont Frejus may be followed by other similar undertakings in other parts of the Alps, the experience here gained may at any rate be turned to account to secure better and more systematic work elsewhere.

NOTE.—Since the above memoir was written, the author has been in correspondence with Prof. Sismonda of Turin, and has obtained permission to repeat the observations recorded, to make corresponding observations in the bore-holes at the French end of the tunnel, and to obtain observations of the temperature at the surface. These observations will be made with instruments provided by the British Association Committee for investigating the rate of increase of underground temperature, and will be conducted by the author, who hopes to visit the tunnel during the present summer.

II. "Some Remarks on the Mechanism of Respiration." By F. LE GROS CLARK, F.R.C.S., Professor of Anatomy and Surgery at the Royal College of Surgeons. Communicated by Prof. P. M. DUNCAN, F.R.S. Received April 18, 1871.

(Abstract.)

The author commences his paper by narrating some experiments on recently slaughtered animals, in the course of which the remarkable tension

of the diaphragm was noticed ; and the varying condition of that muscle, and of the lungs and pleura, with their mutual relations, are commented on.

The importance of this passive tension of the diaphragm is indicated, and exemplified both physiologically and pathologically. It is essential in retaining the supplemental air within the lungs, in restoring the equilibrium of repose, in economizing active muscular power, and in maintaining the pericardial space, &c.

The action of the diaphragm in relation to the walls of the chest and to other muscles is next discussed ; and the influence of the diaphragm in drawing in the chest-walls, under certain circumstances, is pointed out, and illustrated by cases of injury to the spinal cord.

The action of the intercostal muscles, as necessary adjuncts to the diaphragm and as muscles of inspiration, is insisted on and illustrated by diagrams ; and a summary of their action is given.

The agency of the serratus magnus is then discussed ; and reasons are advanced, supported by observation and experiment, to show that it is only under special conditions and to a limited extent that it can be regarded as taking any part in the act of inspiration.

The mobility of the different costal regions and of the sternum is exemplified by observation and experiment.

Lastly, the question of abdominal and thoracic breathing, severally in the male and female, is considered ; and reasons are adduced for concluding that the received opinions on this subject are erroneous.

III. "Researches on the Hydrocarbons of the Series C_nH_{2n+2} ."—VII.

By C. SCHORLEMMER. Communicated by Prof. STOKES, Sec.R.S.

Received April 27, 1871.

In a former communication*, I have shown that the paraffins, the constitution of which is known, may be arranged in four groups. The first group, which I called *normal paraffins*, contain the carbon atoms linked together in a single chain. Of these I have obtained some new ones, which I shall describe more fully in a further communication. The normal paraffins which I have so far studied are given, together with their boiling-points, in the following Table :—

	From petroleum.	From the acids of the series $C_nH_{2n-2}O_4$.	So-called alcohol radicals.	
C_5H_{12}	37°–39°	—	—	—
C_6H_{14}	69°–70°	69°·5	Dipropyl. 69°–70°	From mannite. 71·5°
C_7H_{16}	98°–99°	100°·5	—	—
C_8H_{18}	123°–124°	123°–124°	Dibutyl. 123°–124°	From methyl-hexyl carbinol. 124°

* Proc. Roy. Soc. vol. xvi. p. 367.

That these paraffins have really the constitution which I have ascribed to them follows partly from their mode of formation; thus dipropyl was obtained from the normal propyl iodide, and dibutyl from normal butyl iodide. The constitution of the others was determined by converting them into alcohols and studying the oxidation products of the latter; thus the hexyl hydride from petroleum, as well as that obtained from mannite, was transformed into secondary hexyl alcohol, which on oxidation yielded acetic acid and *normal* butyric acid.

In the communication above referred to, I placed the hydrocarbon C_8H_{18} from methyl-hexyl carbinol amongst another group; but I have found now that this body is identical with dibutyl and also with the hydrocarbon which Zinke obtained from primary octyl alcohol. This chemist prepared also dioctyl, $C_{16}H_{34}$, which consequently is a normal paraffin; and it appears probable that dihexyl, which Brazier and Gossleth obtained by the electrolysis of œnanthylic acid, belongs to this group too.

We are now acquainted with the following normal paraffins:—

	Boiling-points.		Difference.
	Found (mean).	Calculated.	
C H_4	—	—	
C ₂ H_6	—	—	
C ₃ H_8	—	—	
C ₄ H_{10}	1°	1°	
C ₅ H_{12}	38°	38°	37°
C ₆ H_{14}	70°	71°	33°
C ₇ H_{16}	99°	100°	29°
C ₈ H_{18}	124°	125°	25°
C ₁₂ H_{26}	202°	201°	4 × 19°
C ₁₆ H_{34}	278°	278°	4 × 19°

From this it appears that the boiling-point is not raised 31° for each addition of CH_2 , as I formerly assumed, but that, as the calculated numbers show, the difference between the boiling-points of the lower members decreases regularly by 4° until it becomes the well-known difference of 19°.

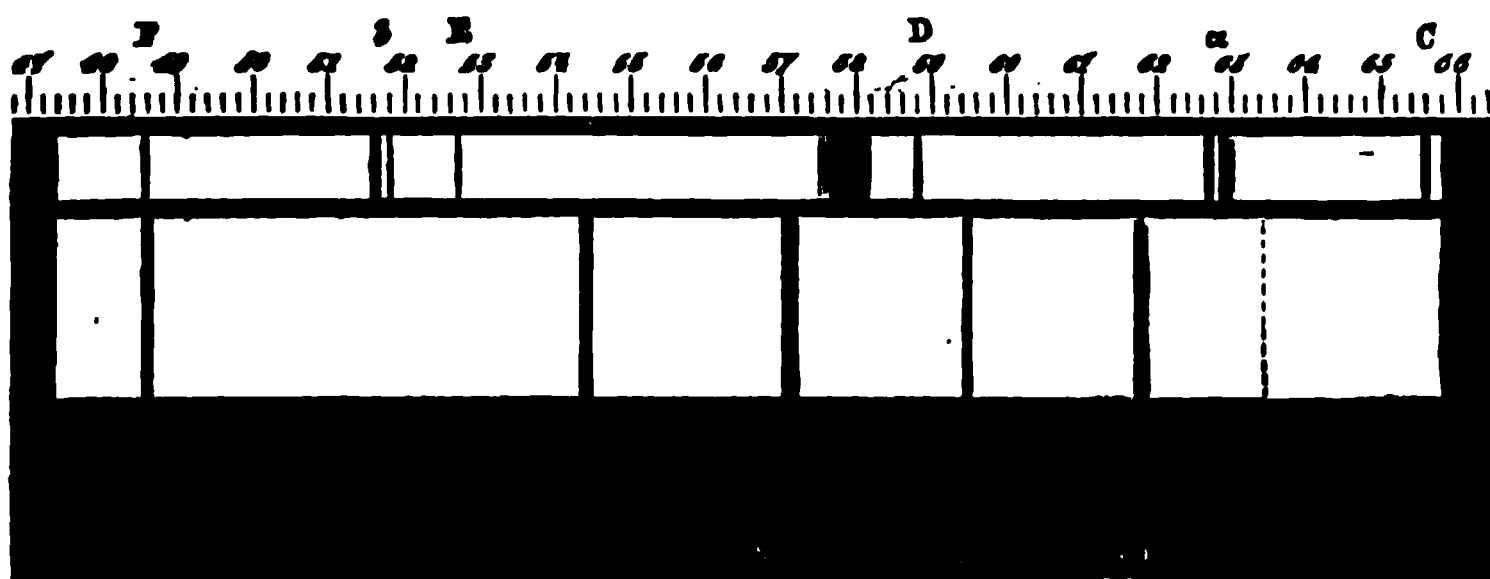
IV. "Note on the Spectrum of Uranus and the Spectrum of Comet I., 1871." By WILLIAM HUGGINS, LL.D., D.C.L., V.P.R.S. Received May 10, 1871.

In the paper "On the Spectra of some of the Fixed Stars"*, presented conjointly by Dr. Miller and myself to the Royal Society in 1864, we gave the results of our observations of the spectra of the planets Venus, Mars, Jupiter, and Saturn; but we found the light from Uranus and Neptune too faint to be satisfactorily examined with the spectroscope.

* *Phil. Trans.* 1864, p. 413; and for Mars, *Monthly Notices R. Astr. Soc.* vol. xxvii. p. 178.

By means of the equatorial refractor of 15 inches aperture, by Messrs. Grubb and Son, recently placed in my hands by the Royal Society, I have succeeded in making the observations described in this paper of the remarkable spectrum which is afforded by the light of the planet Uranus.

It should be stated that the spectrum of Uranus was observed by Father Secchi in 1869*. He says: "le jaune y fait complètement défaut. Dans le vert et dans le bleu il y a deux raies très-larges et très-noires." He represents the band in the blue as more refrangible than F, and the one in the green as near E.



The spectrum of Uranus, as it appears in my instrument, is represented in the accompanying diagram. The narrow spectrum placed above that of Uranus gives the relative positions of the principal solar lines and of the two strongest absorption-bands produced by our atmosphere, namely, the group of lines a little more refrangible than D, and the group which occurs about midway from C to D. The scale placed above gives wave-lengths in millionths of a millimetre.

The spectrum of Uranus is continuous, without any part being wanting, as far as the feebleness of its light permits it to be traced, which is from about C to about G.

On account of the small amount of light received from this planet, I was not able to use a slit sufficiently narrow to bring out the Fraunhofer lines. The positions of the bands produced by planetary absorption, which are broad and strong in comparison with the solar lines, were determined by the micrometer and by direct comparison with the spectra of terrestrial substances.

The spectroscope was furnished with one prism of dense flint-glass, having a refracting-angle of 60° , an observing telescope magnifying $5\frac{1}{2}$ diameters, and a collimator of 5 inches focal length. A cylindrical lens was used to increase the breadth of the spectrum.

The remarkable absorption taking place at Uranus shows itself in six strong lines, which are drawn in the diagram. The least refrangible of these lines occurs in a faint part of the spectrum, and could not be mea-

* *Comptes Rendus*, vol. lxxviii. p. 761, and 'Le Soleil,' Paris, 1870, p. 35A.

sured. Its position was estimated only, and on this account it is represented in the diagram by a dotted line. The positions of the other lines were obtained by micrometrical measures on different nights. The strongest of the lines is that which has a wave-length of about 544 millionths of a millimetre. The band at 572 of the scale is nearly as broad but not so dark; the one a little less refrangible than D is narrower than the others.

The measures taken of the most refrangible band showed that it was at or very near the position of F in the solar spectrum. The light from a tube containing rarefied hydrogen, rendered luminous by the induction-spark, was then compared directly with that of Uranus. The band in the planet's spectrum appeared to be coincident with the bright line of hydrogen.

Three of the bands were shown by the micrometer not to differ greatly in position from some of the bright lines of the spectrum of air. A direct comparison was made, when the principal bright lines were found to have the positions, relatively to the lines of planetary absorption, which are shown in the diagram. The band which has a wave-length of about 572 millionths of a millimetre is less refrangible than the double line of nitrogen which occurs near it. The two planetary bands at 595 and 618 of the scale appeared very nearly coincident with bright lines of air. The faintness of the planet's spectrum did not admit of certainty on this point; I suspected that the planetary lines are in a small degree less refrangible. There is no strong line in the spectrum of Uranus in the position of the strongest of the lines of air, namely, the double line of nitrogen.

As carbonic-acid gas might be considered, without much improbability, to be a constituent of the atmosphere of Uranus, I took measures with the same spectroscope of the principal groups of bright lines which present themselves when the induction-spark is passed through this gas. The result was to show that the bands of Uranus cannot be ascribed to the absorption of this gas.

There is no absorption-band at the position of the line of sodium. It will be seen by a reference to the diagram that there are no lines in the spectrum of Uranus at the positions of the principal groups produced by the absorption of the earth's atmosphere.

Spectrum of Comet I., 1871.

On April 7 a faint comet was discovered by Dr. Winnecke. I observed the comet on April 13 and May 2. On both days the comet was exceedingly faint, and on May 2 it was rendered more difficult to observe by the light of the moon and a faint haze in the atmosphere. It presented the appearance of a small faint coma, with an extension in the direction from the sun.

When observed in the spectroscope, I could detect the light of the coma to consist almost entirely of three bright bands.

A fair measure was obtained of the centre of the middle band, which was the brightest; it gives for this band a wave-length of about 510 millionths of a millimetre. I was not able to do more than estimate roughly the position of the less refrangible band. The result gives 545 millionths. The third band was situated at about the same distance from the middle band on the more refrangible side.

It would appear that this comet is similar in constitution to the comets which I examined in 1868*.

V. "On a New Instrument for recording Minute Variations of Atmospheric Pressure." By WILDMAN WHITEHOUSE, F.M.S. &c. &c. Communicated by R. H. SCOTT, F.R.S., Director of the Meteorological Office. Received May 8, 1871.

(Abstract.)

The occurrence of a heavy "ground-swell" on the sea-coast in perfectly calm weather suggested to the writer some years ago the possibility of atmospheric waves or pulsations accompanying a gale being propagated to a considerable distance (irrespective of any horizontal movement of air), and giving evidence of the disturbance existing elsewhere.

It was seen that, even if such were the case, it would be difficult to obtain proof of it, as any ordinary observations would fail to detect it, and that it could only be attained by the adoption of a system of continuous record specially adapted to the purpose. The writer therefore determined to design and construct an instrument with this object; and after many trials with varied apparatus, it was decided to adopt the hydraulic principle, as affording at once the means of accumulating force sufficient to actuate the instrument, and of measuring the force itself by the alteration produced in the height of the column of water.

The use of an air-chamber was suggested by the sympiesometer; it has, however, been enlarged to meet the altered conditions of this instrument, and is buried underground to secure freedom from all diurnal changes of temperature.

The action of the instrument essentially depends upon the flow and reflow of water between two hydraulic chambers (connected by a tube or siphon), one of which is open and exposed to atmospheric pressure, the other closed at top, and removed from such pressure, being in pneumatic connexion with the buried air-chamber.

Any difference in the levels of the water in these two chambers is a measure of the variation of pressure producing it, and the water in its flow is made to move the tracing-point or pen across the paper.

In order that the objects of the research should be attained, the action must be continuous, unfailing, of great delicacy—able to show changes of

* Phil. Trans. 1868, p. 555; and Proc. Roy. Soc. vol. xvi. p. 386.

$\frac{1}{1000}$ of an inch of mercury and of brief duration, and yet moving with sufficient force to reduce to insignificance the inevitable friction of the pen and working parts.

The chief difficulties met were :—

1st. To retain perfect sensibility to minute variations of pressure without being constantly thrown out of range by the greater changes.

2nd. To record on a very open scale, and yet not to exceed the usual convenient limits of paper and space.

It was found necessary to abandon the idea of recording *absolute* barometric measurements on such a scale, and to deal with minute differences only (hence the name “Differential microbarograph”), making the instrument self-adjusting, so as to act differentially only, allowing brief wave-like motions or pulsations of pressure to record themselves as such, while a steady rise or fall of pressure should record itself by a line or trace, whose mean distance above or below the base-line will indicate pretty accurately the rate per hour of such rise or fall.

This power of self-adjustment has been obtained by use of a capillary tube communicating with the air-chamber and with the atmosphere, whereby the equilibrium disturbed by changes of pressure is being constantly restored, and the pen brought back to the zero.

It is as if the ordinary barographic curve of pressure were made to serve as the base-line, and these minute variations were made to record themselves above or below it, as though they were ripples or waves upon its surface.

The two hydraulic chambers connected by a siphon being in a state of equilibrium, and the closed one being in pneumatic connexion with the buried air-chamber, it is obvious that any change in the atmospheric pressure exerted upon the water in the open one would disturb the equilibrium and alter the levels, by causing water to pass from one to the other.

The end of the siphon, however, opens into a small cylinder closed at the bottom, and suspended in the water in the open chamber; this responds to the movement, and measures the flow of the water taking place between the chambers, the degree of its immersion depending upon the quantity of water it contains, and altering by its displacement the level of the water in which it floats.

Inverted in the water, and attached by a couple of silk lines passing over pulleys to this cylinder, partly as a counterpoise, but also to cooperate with it in producing the movements of the instrument, is another cylinder of equal area, capacity, and weight, closed at top, where it is subjected to the pressure of the atmosphere, open at bottom, where its interior is removed from such pressure by being suspended over the mouth of a tube in pneumatic connexion with the buried air-chamber.

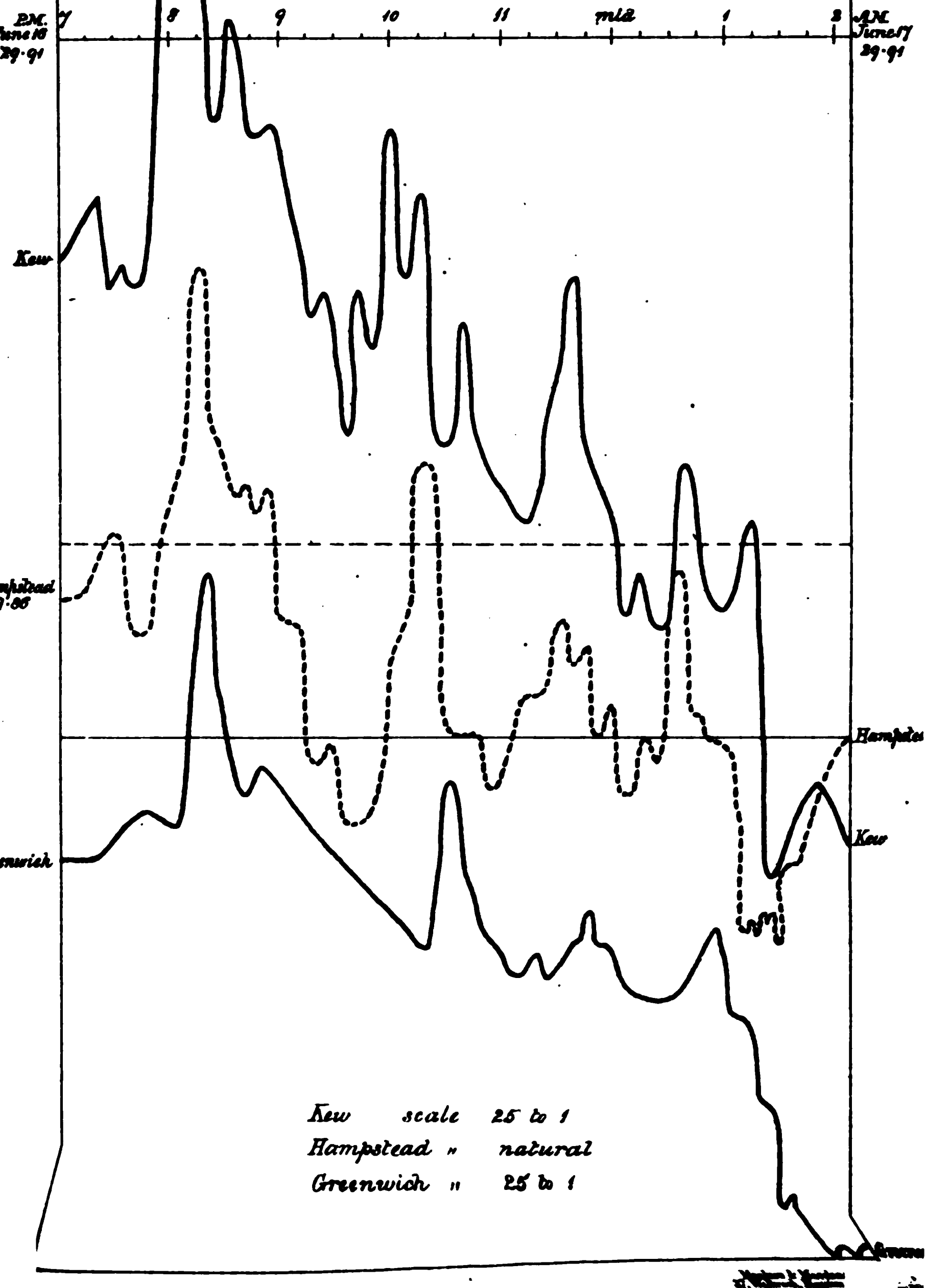
These two cylinders, carefully balanced and suspended half immersed in the water in the open vessel, are equally but oppositely acted upon by any

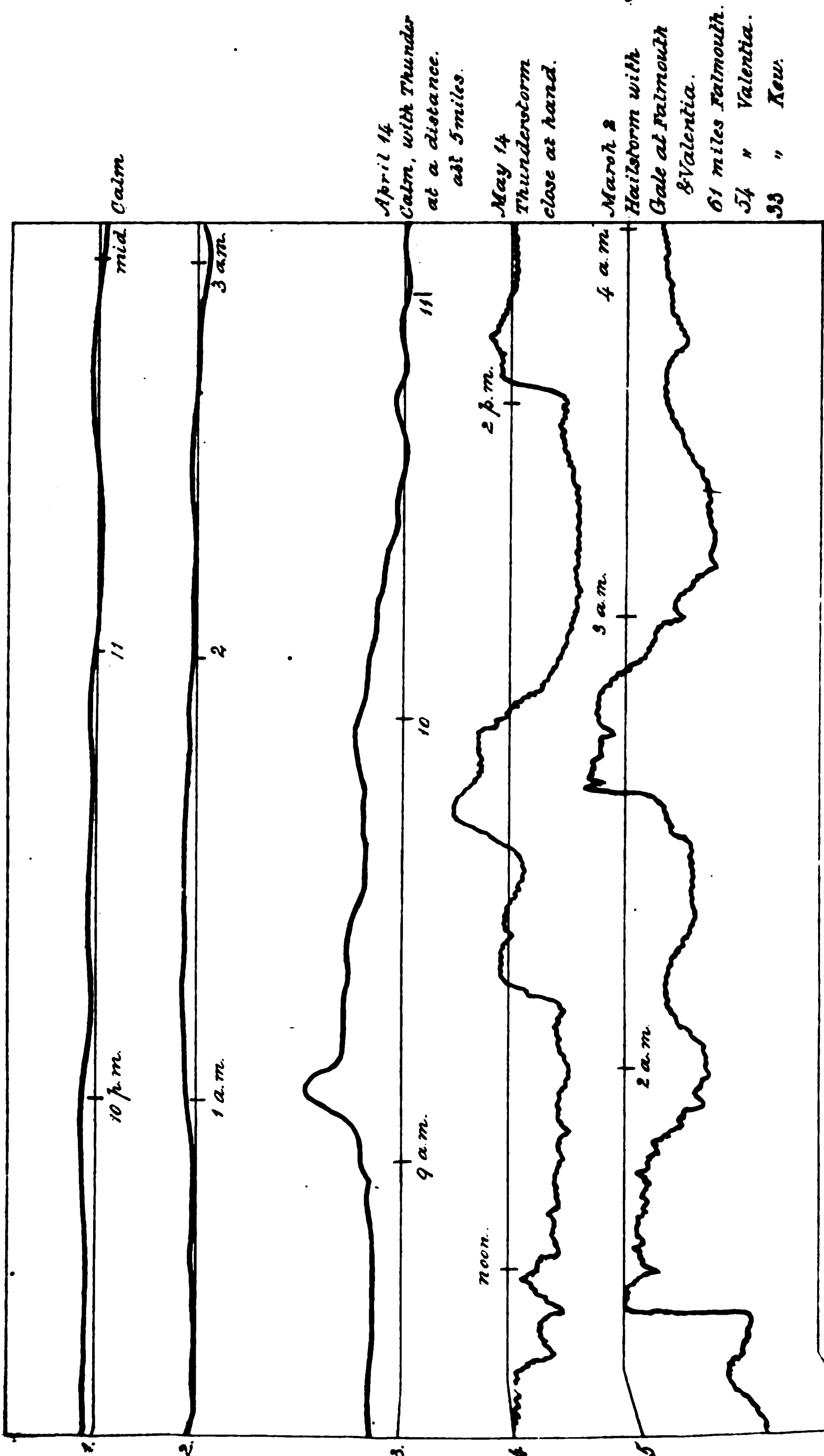


*Specimens of Barograms,
by Mr. W. Whitehouse.*

June 16, 17

1870







change of pressure, the one rising while the other descends, and so combining to produce the desired movement.

A light but rigid V-shaped bar is laid inverted on the sharp edge of a disk placed on the axis of the pulleys supporting the cylinders, the other end of the bar running upon a light disk as a friction-roller.

This bar carries a glass capillary siphon pen of very simple form lightly poised, and having a reservoir of fluid ink sufficient for a month's use; this traverses the paper on the drum in the usual manner.

The relative areas of the cylinders and the hydraulic chambers respectively determine the scale upon which the curves shall be projected; the ratio of 10 to 1 which has been adopted giving half an inch rise or fall for one-tenth of an inch difference in the water-level in the two chambers.

The instrument therefore multiplies by 5, and the difference in the specific gravity of mercury and water again multiplies by 13.59, say $13.6 \times 5 = 68$; this is the scale upon which the curves would be projected were it not for the power of self-adjustment given to the instrument, in order to keep it within range.

By use of an instrument such as described, the writer has at intervals during the last four years accumulated barograms for subsequent examination and discussion, amounting to over 450 days.

The autographs of different days are as diverse as possible, and sometimes most characteristic; tracings of a few types are appended.

It is only within the last few weeks that the writer has been able to commence the comparison of his barograms with independent data gathered from a wide area of official observations.

By the courtesy of the Meteorological Committee of the Royal Society, and the kindness of Mr. Scott, the Director of the Meteorological Office, he has been furnished with all the reports and data at their disposal, which he trusts will enable him thoroughly to discuss the matter.

Meantime the interesting nature of some of the results, the coincidence in point of date between some of the most striking of the micro-barograms, and the existence of storm and gale within a certain radius are not a little remarkable, and are indeed sufficient to induce the writer to lay the matter before the Royal Society in its early stage, rather than await a fuller development under his own hands, in order to insure for it a wider basis than his own individual observations alone could command.

Personally the writer entertains little doubt that these barograms show the existence, under some circumstances, of the atmospheric storm-waves of which he has been in search, the most marked feature in such case being the rhythmical character and strikingly wave-like form which the minute movements assume.

It is believed that much information may result from such additional means of research, and the writer offers the instrument in aid of meteorological science.

The Society adjourned over the Whitsuntide Recess to Thursday, June 15.

June 8, 1871.

The Annual Meeting for the election of Fellows was held this day.

Sir PHILIP GREY-EGERTON, Bart., Vice-President, followed by Mr. W. SPOTTISWOODE, Treasurer and Vice-President, in the Chair.

The Statutes relating to the election of Fellows having been read, Dr. Allman and Mr. W. H. L. Russell were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present having been collected, the following Candidates were declared to be duly elected into the Society:—

William Henry Besant, M.A.

William Budd, M.D.

George William Callender, F.R.C.S.

William Carruthers, Esq.

Robert Etheridge, F.R.S.E.

Frederick Guthrie, B.A.

John Herschel, Capt. R.E.

Alexander Moncrieff, Capt. M.A.

Richard Quain, M.D.

Carl Schorlemmer, Esq.

Edward Thomas, Esq.

Edward Burnet Tylor, Esq.

Cromwell Fleetwood Varley, C.E.

Arthur Viscount Walden, P.Z.S.

John Wood, F.R.C.S.

Thanks were voted to the Scrutators.

June 15, 1871.

General Sir EDWARD SABINE, K.C.B., President, in the Chair.

Mr. W. H. Besant, Mr. G. W. Callender, Mr. W. Carruthers, Mr. R. Etheridge, Prof. F. Guthrie, Right Hon. R. Lowe, Capt. A. Moncrieff, Dr. R. Quain, Mr. E. Thomas, Viscount Walden, and Mr. J. Wood, were admitted into the Society.

The following communications were read:—

- I. "On the Fossil Mammals of Australia.—Part V. Genus *Nototherium*, Ow." By Prof. R. OWEN, F.R.S. Received May 8, 1871.

(Abstract.)

The genus of large extinct Marsupial herbivore which forms the subject of the present paper was founded on specimens transmitted (in 1842) to the author by the Surveyor-General of Australia, Sir Thomas Mitchell, C.B. They consisted of mutilated fossil mandibles and teeth. Subsequent specimens confirmed the distinction of *Nototherium* from *Diprotodon*, and more especially exemplified a singular and extreme modification of the cranium of the former genus. A detailed description is given of this part

from specimens of portions of the skull in the British Museum, and from a cast and photographs of the entire cranium in the Australian Museum at Sydney, New South Wales. The descriptions of the mandible, and of the dentition in both upper and lower jaws, are taken from actual specimens in the British Museum, in the Museum of Natural History at Worcester, and in the Museum at Adelaide, S. Australia, all of which have been confided to the author for this purpose. The results of comparisons of these fossils of *Nototherium* with the answerable parts in *Diprotodon*, *Macropus*, *Phascolarctos*, and *Phascolomys* are detailed.

Characters of three species, *Nototherium Mitchelli*, *N. inerme*, and *N. Victoriae*, are defined chiefly from modifications of the mandible and mandibular molars. A table of the localities where fossils of *Nototherium* have been found, with the dates of discovery and names of the finders or donors, is appended. The paper is illustrated by subjects for nine quarto Plates.

II. "On Cyclides and Sphero-Quartics." By JOHN CASEY, LL.D., M.R.I.A. Communicated by Prof. CAYLEY, F.R.S. Received May 11, 1871.

(Abstract.)

The curves and surfaces considered in this paper are, I believe, some of the most fertile in properties in the whole range of geometry. For the purpose of giving a full and comprehensive discussion, I have divided the paper into several chapters. The following is an outline of the method of investigation pursued, together with a statement of some of the results arrived at.

If we take the most general equation of the second degree in $(\alpha, \beta, \gamma, \delta)$, where these variables denote spheres instead of planes,

$$(a \ b \ c \ d \ l \ m \ n \ p \ q \ r) \chi(\alpha, \beta, \gamma, \delta)^2 = 0,$$

we get the most general form in which the equation of a quartic cyclide can be written. Setting out with this equation, I have proved that a quartic cyclide is the envelope of a variable sphere, whose centre moves on a given quadric, and which cuts orthogonally the Jacobian of the spheres of reference $(\alpha, \beta, \gamma, \delta)$.

The Jacobian of $(\alpha, \beta, \gamma, \delta)$ can be written in a form identical with that of the imaginary circle at infinity in the system of quadriplanar coordinates. The square of the Jacobian can be expressed by an equation of the second degree in $\alpha, \beta, \gamma, \delta$. This equation assumes a very simple form when $\alpha, \beta, \gamma, \delta$ are mutually orthogonal. By means of it I have shown that every quartic cyclide can be written in the canonical form,

$$a\alpha^2 + b\beta^2 + c\gamma^2 + d\delta^2 + e\epsilon^2 = 0,$$

where $\alpha, \beta, \gamma, \delta, \epsilon$ are five spheres mutually orthogonal. These are spheres of inversion of the cyclide, and by incorporating constants their equations are connected by an identical relation, $\alpha^2 + \beta^2 + \gamma^2 + \delta^2 + \epsilon^2 = 0$.

From these equations I have shown that in general a quartic can be generated in five different ways as the envelope of a variable sphere which cuts a given sphere orthogonally, and whose centre moves on a given quadric, which, on account of one of its most important properties, I have named the *focal quadric* of the cyclide. Every cyclide has, in general, five focal quadrics; these focal quadrics are confocal; their focal conics are double, or “nodo-foci” of the cyclide.

I have shown that the locus of the single or ordinary foci of cyclides are sphero-quartics (curves of intersection of a sphere and a quadric). In general a cyclide has five focal sphero-quartics. If we call confocal two cyclides having in common one focal sphero-quartic, through any point can be described three cyclides confocal with a given cyclide. These confocals are mutually orthogonal. Other methods of generating cyclides are also given; thus three circles in space being given, whose planes are diametral planes of a given sphere, and which are orthogonal to the sphere, a cyclide will be generated by a variable circle in space which rests on these three circles. This method is analogous to that for describing ruled quadrics by the motion of a line. The equation of a cyclide may be interpreted in three different ways:—1, so as to denote a cyclide; 2, a sphero-quartic; 3, a tangent cone to the cyclide. Hence it follows that sphero-quartics, both in their modes of generation and in many of their properties, bear a striking analogy to cyclides. Thus the canonical form of the equation of a sphero-quartic is $a\alpha^2 + b\beta^2 + c\gamma^2 + d\delta^2 = 0$, where $\alpha, \beta, \gamma, \delta$ are circles on a given sphere U ; the poles of the planes of $\alpha, \beta, \gamma, \delta$ with respect to U are the vertices of the four cones which can be described through the sphero-quartic. The equations of $\alpha, \beta, \gamma, \delta$ are, by incorporating constants, connected by an identical relation, $\alpha^2 + \beta^2 + \gamma^2 + \delta^2 = 0$. By means of this relation, which holds also for bicircular quartics, I have got the equations of the four focal sphero-conics of the sphero-quartic. These sphero-conics are constructed geometrically as the intersections with U of perpendiculars from its centre on the tangent-planes to the four cones which can be drawn through the sphero-quartic. The focal sphero-conics are confocal, their foci being the double or nodo-foci of the sphero-quartic.

Sphero-quartics may be inverted into bicirculars; they may also be projected into bicirculars, and that in two ways. First, on either plane of circular section of the quadric, whose intersection with the sphere is the sphero-quartic by lines parallel to the greatest or least axis of the quadric; second, by elliptic projection—that is, by lines of curvature of confocal quadrics passing through each point of the sphero-quartic. The developable formed by tangent planes to the sphere U , at every point of the sphero-quartic, possesses many geometrical properties. Thus the cone whose vertex is at the centre of U , and which stands on its cuspidal edge, may be generated by the focal lines of a variable cone osculating one cone of the second degree, and having double contact with another. The cuspidal edge and the nodal lines of the developable may be projected

into the evolute and the focal conics of a bicircular quartic. The developable possesses numerous anharmonic properties; thus all its generators are divided homographically by the nodal lines and the sphere U .

In the chapters on the inversion and classification of cyclides, I have proved that the presence or absence of nodes depends on the relative positions of the focal quadric and sphere of inversion; thus if they touch there will be a conic node, the cyclide being in this case the inverse of a quadric, which is an hyperboloid or ellipsoid according as the node has a real or imaginary cone of contact. If they osculate, the cyclide will be the inverse of a paraboloid; the node will be biplanar if the paraboloid be an elliptic or hyperbolic one, and it will be uniplanar if the paraboloid be cylindrical. If the focal quadric and sphere of inversion have double contact, the cyclide will be the inverse of a cone of the second degree, and will have two nodes, which must be conic nodes. When a cyclide has nodes, the number of focal quadrics suffers diminution. I have given in the same chapters the equations and the singularities of the tangent cones, and shown that in general every cyclide has as many double tangent cones as it has focal quadrics; in fact the double tangent cones are the reciprocals of the asymptotic cones of the focal quadrics. It is also proved that the lines of intersection of a cyclide, with its spheres of inversion, are lines of curvature on the cyclide, and that the imaginary circle at infinity is a flecnodal curve on its surface of centres.

In the chapter on the classification of sphero-quartics I have given Charles's characteristics for the osculating circles of a sphero-quartic. By inversion we get the characteristics for the osculating circles of bicircular quartics. Thus $V=24$ for these circles. In the same chapter Professor Cayley's equations, giving the singularities of the cuspidal edges of developables, are transformed so as to give the singularities of the evolute of a plane curve, any three of the singularities of the curve being given.

The last two chapters contain an account of the substitutions by which, from properties of quadrics, may be inferred corresponding properties of cyclides. These chapters are in reality an exposition of a new method of geometrical transformation; in fact, since the general equation in $\alpha, \beta, \gamma, \delta$ which I employ is the same in form as the general equation of a quadric, only that in my method the variables denote spheres in place of planes, it will be readily seen that the theories of invariants, reciprocation, &c. in the geometry of surfaces of the second degree have their analogues in the theory of cyclides, and, in fact, the modes of proof employed in one apply also in the other. This method of transformation is very fertile; I have illustrated it by numerous theorems. Thus the locus of the centre of a variable sphere cutting in two sphero-quartics having double contact, two cyclides having a common sphere of inversion is the developable circumscribed about the focal quadrics of these cyclides, which correspond to the common sphere of inversion.

III. "On a Law in Chemical Dynamics." By JOHN HALL GLADSTONE, Ph.D., F.R.S., and ALFRED TRIBE, F.C.S. Received May 25, 1871.

(Abstract.)

It is well known that one metal has the power of decomposing the salts of certain other metals, and that the chemical change will proceed until the more powerful metal has entirely taken the place of the other. The authors have investigated what takes place during the process.

The experiments were generally performed as follows:—72 cubic centimetres of an aqueous solution of the salt of known strength, and at 12° Centigrade, were placed in a tall glass; a perfectly clean plate of metal of 3230 square millimetres was weighed and placed vertically in this solution without reaching either to the top or bottom; the action was allowed to proceed quietly for ten minutes, when the plate was removed, and the deposited metal was washed off. The loss of weight gave the amount of metal dissolved, and represented the chemical action.

The most complete series of results was with copper and nitrate of silver.

Nitrate-of-silver solution.		Copper dissolved.		Theoretical.	Difference.
Proportional number.	Percentage of salt.	Actual weights.	Average.		
1.	0.3541	0.0045, 0.0050	0.00475	0.00455	+0.0002
2.	0.7083	0.0135, 0.0140	0.01375	0.01385	+0.0001
3.	1.0623	0.0240, 0.0250	0.0245	0.0259	−0.0014
4.	1.4166	0.0420	0.0420	0.0409	+0.0011
5.	1.7705	0.0600	0.0600	0.0583	+0.0017
6.	2.1246	0.0785	0.0785	0.0790	−0.0005
7.	2.4788	0.0975	0.0975	0.0994	−0.0019
8.	2.8332	0.1230, 0.1230	0.1230	0.1228	+0.0002
9.	3.1873	0.1510, 0.1480	0.1495	0.1481	+0.0014
10.	3.5415	0.1680, 0.1670	0.1675	0.1749	−0.0074
11.	3.8956	0.1955	0.1955	0.2035	−0.0080
12.	4.2497	0.2170, 0.2285, 0.2310, 0.2200	0.2241	0.2336	−0.0095
14.	4.9580	0.2740	0.2740	0.2982	−0.0242
16.	5.6664	0.3270	0.3270		
20.	7.0830	0.4540, 0.4100	0.4320		
24.	8.4994	0.5400	0.5400		
30.	10.624	0.6850	0.6850		
32.	11.333	0.7100	0.7100		
40.	14.166	0.8440, 0.9090	0.8765		
48.	16.999	1.0690	1.0690		
60.	21.246	1.359	1.359		
70.	24.788	1.580	1.580		

In the earlier terms of this series, *twice the percentage of silver-salt gives three times the chemical action*. The close agreement of the observed numbers with those calculated on this supposition as far as the 9th term

is shown in the 5th and 6th columns. The law then breaks down, and after about 7 per cent. the increased action is almost in direct ratio with the increased strength.

The position of the plate in the solution was found to make no difference to this 2-3 law.

Similar series of experiments were made with zinc and chloride of copper, zinc and sulphate of copper, zinc and nitrate of lead, iron and sulphate of copper, and other combinations; and in every instance where the solution was weak and the action simple, the law of three times the chemical change for twice the strength was found to hold good.

It was proved that the breaking down of the law at about 3·5 per cent. of salt in solution was irrespective of the quantity of the liquid, or of the time for which the plate was exposed. With 72 cub. centims. of a 1·41 per cent. solution of nitrate of silver the rate of action remained sensibly the same for as long as twenty-five minutes, notwithstanding the constant deposition of silver. This apparently paradoxical result is due to fresh relays of the original solution being brought up to the plate by the currents produced, and that period of time elapsing before any of the products of decomposition are brought back again in their circuit.

When it was perceived that within easily ascertainable limits the chemical action is the same for similar consecutive periods of time, experiments were made in far weaker solutions. It was only necessary to lengthen the time of exposure. It was thus found that the law of three times the chemical action for twice the strength of solution holds good through at least eleven terms of the powers of 2; in fact, from a solution that could dissolve one gramme of copper during the hour, to a solution that dissolved only 0·000001 gramme, a million times less.

The manner in which the silver is deposited on a copper plate was examined, and the currents produced were studied. At first a light blue current is perceived flowing upwards from the surface of the plate, presently a deep blue current pours downwards, and these two currents in opposite directions continue to form simultaneously. A similar phenomenon was observed in every case where a metallic salt attacked a plate of another metal. The downward current was found to be a solution of almost pure nitrate of copper, containing about three times as much NO_3 as the original silver solution, while the upward current was a diluted solution of the mixed nitrates. Moreover the heavy current took its rise in the entangled mass of crystals right against the plate, while the light current flowed from the tops of the crystalline branches. It was evident that when the fresh silver was deposited on these branches, and the fresh copper taken up from the plate, there was not merely a transference of the nitric element from one combination to another, but an actual molecular movement of it towards the copper plate, producing an accumulation of nitrate of copper there, and a corresponding loss of salt in the liquid that is drawn within the influence of the branching crystals. Hence the opposite currents.

The amount of action in a circuit of two metals and a saline solution must have as one of its regulating conditions the conducting-power of that solution. It appeared by experiment that a strong solution of nitrate of silver offers less resistance than a weak one; and it was also found, on adding nitrate of potassium to the nitrate of silver, that its power of attacking the copper plate was increased, that the augmentation of the foreign salt increased the action still further, and that the 2-3 law holds good between two solutions in which both the silver and potassium salt are doubled, though it does not hold good if the quantity of foreign salt be kept constant. Similar results were obtained with mixed nitrates of silver and copper.

While these later experiments offer an explanation of the fact that a solution of double the strength produces more than double the chemical action, they do not explain why it should produce exactly three times the effect, or why the ratio should be the same in all substitutions of this nature hitherto tried. The simplicity and wide range of the 2-3 law seem to indicate that it is a very primary one in chemical dynamics.

IV. "On the Organization of the Fossil Plants of the Coal-measures.—
Part II. *Lepidodendra and Sigillariæ*." By W. C. WILLIAMSON,
F.R.S., Professor of Natural History in Owens College, Man-
chester. Received June 13, 1871.

(Abstract.)

The *Lepidodendron selaginoides* described by Mr. Binney, and still more recently by Mr. Carruthers, is taken as the standard of comparison for numerous other forms. It consists of a central medullary axis composed of a combination of transversely barred vessels with similarly barred cells; the vessels are arranged without any special linear order. This tissue is closely surrounded by a second and narrower ring, also of barred vessels, but of smaller size, and arranged in vertical laminæ which radiate from within outwards. These laminæ are separated by short vertical piles of cells, believed to be medullary rays. In the transverse section the intersected mouths of the vessels form radiating lines, and the whole structure is regarded as an early type of an exogenous cylinder; it is from this cylinder alone that the vascular bundles going to the leaves are given off. This woody zone is surrounded by a very thick cortical layer, which is parenchymatous at its inner part, the cells being without definite order; but externally they become prosenchymatous, and are arranged in radiating lines, which latter tendency is observed to manifest itself whenever the bark-cells assume the prosenchymatous type. Outside the bark is an epidermal layer, separated from the rest of the bark by a thin bast-layer of prosenchyma, the cells of which are developed into a tubular and almost vascular form; but the vessels are never barred, being essentially of the fibrous type.

Externally to this bast-layer is a more superficial epiderm of parenchyma, supporting the bases of leaves, which consist of similar parenchymatous tissue. Tangential sections of these outer cortical tissues show that the so-called "decorticated" specimens of *Lepidodendra* and of other allied plants are merely examples that have lost their epidermal layer or had it converted into coal, this layer, strengthened by the bast-tissue of its inner surface, having remained as a hollow cylinder when all the more internal structures had been destroyed or removed.

From this type the author proceeds upwards through a series of examples in which the *vessels* of the medulla become separated from its central *cellular* portions and retreat towards its periphery, forming an outer cylinder of medullary vessels, which are arranged without order and enclose a defined cellular axis; at the same time the encircling ligneous zone of radiating vessels becomes yet more developed, both in the number of its vessels and in the diameter of the cylinder relatively to that of the entire stem. As these changes are produced, the medullary rays separating the laminæ of the woody wedges become more definite, some of them assuming a more composite structure, and the entire organization gradually assuming a more exogenous type; at the same time the cortical portions retain all the essential features of the *Lepidodendroid* plants. Commencing with the *Lepidodendron selaginoides* just described, we pass on to *L. Harcourtii*, in which there is a distinct cellular axis to the medulla, surrounded by a ring of medullary vessels, externally to which is the second or radiating cylinder of vessels, from which alone, as M. Brongniart has very correctly shown, the bundles of vessels supplying the leaves are derived. Then we reach the more highly organized of the forms which Mr. Binney has described under the common name of *Sigillaria vascularis*, in which the woody cylinder is more extensively developed. This conducts us to a series of varieties from which the cells of the medulla have disappeared, but in which there is a very distinct inner cylinder of large barred vessels not arranged in radiating order, and an outer and much more ample cylinder of smaller ones arranged on the exogenous type. In these examples the line of demarcation between the vessels of the medulla and those of the ligneous zone is sometimes straight and at others boldly crenulated. In the latter examples the outside of the vascular medullary cylinder, detached from its surroundings, exhibits the fluted appearance of a *Calamite*, for which it might be mistaken, but it lacks the transverse nodal constrictions of that genus. It is to some of these more highly organized *Lepidodendra* just referred to that Corda has applied the name of *Diploxyton*, and Witham that of *Anabathra*, both of which correspond in the closest manner with the *Sigillaria elegans* of M. Brongniart. We are thus brought, by the evidence of internal organization, to the conclusion that the plants which Brongniart has divided into two distinct groups, the one of which he has placed amongst the vascular Cryptogams, and the other amongst the Gymnospermous Exogens, constitute one great natural family.

Of this family numerous other modifications are described. Thus *Ulodendron* and *Halonias*, very closely allied, if not identical genera, have a structure closely corresponding with that of *Lepidodendron Harcourtii*, since they possess a very distinct cellular medullary axis enclosed within the ring of medullary vessels, and, besides, exhibit the enclosing ligneous zone at its minimum stage of development. The remarkable scars of *Ulodendron* and the tubercles of *Halonias* appear to have had their most prominent surfaces composed of the true bark-layer deprived of its epidermal bast and parenchymatous layers, which surround these structures but do not wholly enclose them. These characteristic structures are believed to have supported special organs, into which the epidermal layer of the stem has been prolonged, and which the author believes to have been reproductive cones. *Favularia* corresponds very closely, so far as its cortical layer is concerned, with those already described; and as Brongniart's *Sigillaria elegans* is an unquestionable *Favularia*, the entire series of this subgenus is brought into the closest relationship with the plants described. But the author has further met with some important examples, showing that the stem supported verticils of organs that were neither leaves nor branches, but which are believed to have been cones, thus bringing to light an additional indication of affinity between *Favularia*, *Halonias*, and *Ulodendron*.

Well-marked examples have also been obtained from the Lancashire Lower Coal-measures, the source whence all the specimens described have been obtained, of the outer cortical layers of true *Sigillaria*. These specimens demonstrate that the bark of these plants is of the true *Lepidodendron* type. No example of an unquestionable *Sigillaria* in which the central woody axis is preserved has yet been seen by the author.

Stigmaria is shown to have been much misunderstood, so far as the details of its structure are concerned, especially of late years. In his memoir of *Sigillaria elegans*, published in 1839, M. Brongniart gave a description of it, which, though limited to a small portion of its structure, was, as far as it went, a remarkably correct one. The plant now well known to be a root of *Sigillaria*, possessed a cellular pith without any trace of a distinct outer zone of medullary vessels, such as is universal amongst the *Lepidodendron*. The pith is immediately surrounded by a thick and well-developed ligneous cylinder, which contains two distinct sets of primary and secondary medullary rays. The primary ones are of large size, and are arranged in regular quincuncial order; they are composed of thick masses of mural cellular tissue. A tangential section of each ray exhibits a lenticular outline, the long axis of which corresponds with that of the stem. These rays pass directly outwards from pith to bark, and separate the larger woody wedges which constitute so distinct a feature in all transverse sections of this zone, and each of which consists of aggregated laminæ of barred vessels disposed in very regular radiating series. The smaller rays consist of vertical piles of cells, arranged in single rows, and often consisting of but one, two, or three cells in each vertical series;

these latter are very numerous and intervene between all the numerous radiating laminæ of vessels that constitute the larger wedges of woody tissue. The vessels going to the rootlets are not given off from the pith, as Goeppert supposed, but from the sides of the woody wedges bounding the *upper* part of the several large lenticular medullary rays, those of the *lower* portion of the ray taking no part in the constitution of the vascular bundles. The vessels of the region in question descend vertically and parallel to each other until they come in contact with the medullary ray, when they are suddenly deflected, in large numbers, in an outward direction, and nearly at right angles to their previous course, to reach the rootlets. But only a small number reach their destination, the great majority of the deflected vessels terminating in the woody zone. A very thick bark surrounds the woody zone. Immediately in contact with the latter it consists of a thin layer of delicate vertically elongated cellular tissue, in which the mural tissues of the outer extremities of the medullary rays become merged. Externally to this structure is a thick parenchyma, which quickly assumes a more or less prosenchymatous form and becomes arranged in thin radiating laminæ as it extends outwards. The epidermal layer consists of cellular parenchyma with vertically elongated cells at its inner surface, which feebly represents the bast-layer of the other forms of Lepidodendroid plants. The rootlets consist of an outer layer of parenchyma, derived from the epidermal parenchyma. Within this is a cylindrical space, the tissue of which has always disappeared. In the centre is a bundle of vessels surrounded by a cylinder of very delicate cellular tissue, prolonged either from one of the medullary rays or from the delicate innermost layer of the bark, because it always accompanies the vessels in their progress outwards through the middle and outer barks.

The facts of which the preceding is a summary lead to the conclusion that all the forms of plants described are but modifications of the Lepidodendroid type. The leaf-scars of the specimens so common in the coal-shales represent tangential sections of the petioles of leaves when such sections are made close to the epidermal layer. The thin film of coal of which these leaf-scars consist, in specimens found both in sandstone and in shale, does not represent the entire bark as generally thought, and as is implied in the term "decorticated" usually applied to them, but is derived from the epidermal layer. In such specimens all the more central axial structures (*viz.* the medulla, the wood, and the thick layer of true bark) have disappeared through decay, having been either destroyed or in some instances detached and floated out; the bast-layer of the epiderm has arrested the destruction of the entire cylinder, and formed the mould into which inorganic materials have been introduced. On the other hand, the woody cylinder is the part most frequently preserved in *Stigmaria*; doubtless because, being subterranean, it was protected against the atmospheric action which destroyed so much of the stem.

It is evident that all these Lepidodendroid and Sigillarian plants must

be included in one common family, and that the separation of the latter from the former as a group of Gymnosperms, as suggested by M. Brongniart, must be abandoned. The remarkable development of exogenous woody structures in most members of the entire family indicates the necessity of ceasing to apply either to them or to their living representatives the term Acrogenous. Hence the author proposes a division of the vascular Cryptogams into an exogenous group, containing *Lycopodiaceæ*, *Equisetaceæ*, and the fossil *Calamitaceæ*, and an endogenous group, containing the ferns; the former uniting the Cryptogams with the Exogens through the *Cycadeæ* and other Gymnosperms, and the latter linking them with the Endogens through the *Palmaceæ*.

V. "Contributions to the History of the Opium Alkaloids.—Part II. On the Action of Hydrobromic Acid on Codeia and its derivatives." By C. R. A. WRIGHT, D.Sc., Lecturer on Chemistry in St. Mary's Hospital Medical School. Communicated by Dr. H. E. Roscoe. Received June 7, 1871.

It has been shown in Part I. of this research * that the action of hydrobromic acid on codeia gives rise, without evolution of methyl bromide, firstly to bromocodide, and secondly to two other new bases termed respectively deoxycodide and bromotetracodide, the latter of which, under the influence of hydrochloric acid, exchanges bromine for chlorine, yielding a corresponding chlorinated base, chlorotetracodide; when, however, the action of hydrobromic acid is prolonged, methyl bromide is evolved in some little quantity. By digesting codeia with three or four times its weight of 48 per cent. acid for five or six hours on the water-bath, vapours were evolved which condensed by the application of a freezing-mixture to a colourless mobile liquid, the boiling-point of which was found to be $10^{\circ}5$ to $11^{\circ}5$, and the vapour of which burnt with a yellow-edged flame, and exploded violently with oxygen, forming carbonic and hydrobromic acids. It becomes, therefore, of interest to examine in detail the action of hydrobromic acid on each of the three bodies produced from codeia under its influence.

§ 1. *Action of Hydrobromic Acid on Bromotetracodide.*

When bromotetracodide hydrobromate is heated in a sealed tube to 100° with four or five times its weight of 48 per cent. hydrobromic acid for from six to ten hours, methyl bromide is found as a thin layer on the top of the tarry contents of the tube after cooling; by dissolving this tarry substance in water and fractionally precipitating the liquid by strong hydrobromic acid several times successively, nearly white amorphous flakes are ultimately obtained, resembling in all their physical and chemical properties the bromotetracodide hydrobromate originally employed. After desiccation, first

* Proc. Roy. Soc. vol. xix. p. 371.

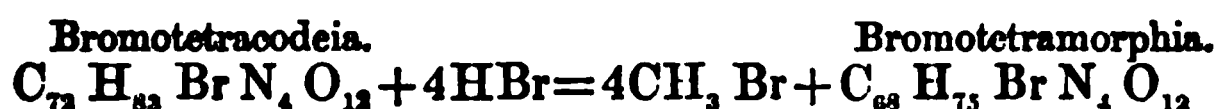
over $\text{SO}_4 \text{H}_2$ and finally at 100° , there were obtained the following numbers, which correspond with those required for a base bearing the same relation to morphia that bromotetracodeia does to codeia; it is therefore provisionally named bromotetramorphia.

0.3110 grm. gave 0.5980 CO_2 and 0.1470 $\text{H}_2 \text{O}$.*

0.2785 grm. gave 0.1650 AgBr .

	Calculated.		Found.	
C_{68}	816	52.88	52.44	
H_{79}	79	5.12	5.25	
Br_5	400	25.93	25.22
N_4	56	3.63		
O_{12}	192	12.44		
$\text{C}_{68} \text{H}_{79} \text{Br}_5 \text{N}_4 \text{O}_{12}, 4\text{HBr}$	1543	100.00		

Hence the action of hydrobromic acid on bromotetracodeia is



Carbonate of soda throws down from the solution of the hydrobromate a nearly white precipitate, which rapidly oxidizes and appears identical in all its physical properties and chemical reactions with bromotetracodeia.

When crude bromotetramorphia hydrobromate is precipitated by carbonate of soda and the precipitate (after filtration and washing) redissolved in hydrochloric acid and fractionally precipitated twice or thrice by strong hydrochloric acid, white flakes free from bromine are ultimately obtained; these are the hydrochlorate of the corresponding chlorinated base, which is therefore termed chlorotetramorphia. After drying at 100° the following numbers were obtained:—

0.4005 grm. gave 0.9030 CO_2 and 0.2230 $\text{H}_2 \text{O}$.

0.4670 grm. gave 0.2280 AgCl and 0.0180 Ag .

	Calculated.		Found.	
C_{68}	816	61.79	61.49	
H_{79}	79	5.98	6.19	
Cl_5	177.5	13.44	13.35
N_4	56	4.24		
O_{12}	192	14.55		
$\text{C}_{68} \text{H}_{79} \text{Cl N}_4 \text{O}_{12}, 4\text{HCl}$	1320.5	100.00		

Converted into platinum-salt and dried at 100° ,—

0.4235 grm. gave 0.0840 Pt = 19.83 per cent.

The formula $\text{C}_{68} \text{H}_{79} \text{Cl N}_4 \text{O}_{12}, 4\text{HCl}, 2\text{PtCl}_4$ requires 19.72 per cent.

* All combustions given in this paper were made by lead chromate and oxygen; except where otherwise stated, chlorine and bromine were determined by boiling with silver nitrate and nitric acid.

When codeia is heated on the water-bath with three parts of 48 per cent. hydrobromic acid for five hours, and the portion of the precipitate thrown down by carbonate of soda and insoluble in ether is dissolved in hydrochloric acid and fractionally precipitated several times by excess of stronger acid, flakes are obtained which, on drying at 100° , yield numbers intermediate between those required for chlorotetracodeia and chlorotetramorphia.

0.3365 grm. gave 0.7670 CO_2 and 0.1950 H_2O .

0.7520 grm. burnt with quicklime gave 0.4100 Ag Cl.

	Calculated.		Found.	
C_{70}	840	62.30	62.17	..
H_{43}	83	6.15	6.44	
Cl_5	177.5	13.16	13.48
N_4	56	4.15		
O_{12}	192	14.24		
$\text{C}_{70} \text{H}_{79} \text{ClN}_4 \text{O}_{12}, 4\text{HCl}$	1348.5	100.00		

Converted into platinum-salt and dried at 100° :—

0.4830 grm. gave 0.0935 Pt = 19.36 per cent.

The formula $\text{C}_{70} \text{H}_{79} \text{ClN}_4 \text{O}_{12}, 4\text{HCl}, 2\text{PtCl}_4$ requires 19.40 per cent.

Whether this is only a mixture of chlorotetracodeia and chlorotetramorphia hydrochlorates, or is one compound, is open to doubt ; assuming that it is not a mixture, the name chloro-dicodeia-dimorphia might be applied to the base. It appears *à priori* probable that the following double series of bases should be obtainable by successive methyl eliminations :—

Bromotetracodeia	$\text{C}_{72} \text{H}_{83} \text{BrN}_4 \text{O}_{12}$	Chlorotetracodeia	$\text{C}_{72} \text{H}_{83} \text{ClN}_4 \text{O}_{12}$
	$\text{C}_{71} \text{H}_{81} \text{BrN}_4 \text{O}_{12}$		$\text{C}_{71} \text{H}_{81} \text{ClN}_4 \text{O}_{12}$
	$\text{C}_{70} \text{H}_{79} \text{BrN}_4 \text{O}_{12}$	Chloro-dicodeia-di-	$\text{C}_{70} \text{H}_{79} \text{ClN}_4 \text{O}_{12}$
	$\text{C}_{69} \text{H}_{77} \text{BrN}_4 \text{O}_{12}$	morphia	$\text{C}_{69} \text{H}_{77} \text{ClN}_4 \text{O}_{12}$
Bromotetramorphia	$\text{C}_{69} \text{H}_{75} \text{BrN}_4 \text{O}_{12}$	Chlorotetramorphia	$\text{C}_{69} \text{H}_{75} \text{ClN}_4 \text{O}_{12}$

Out of these ten bases four have been prepared, and a substance corresponding in composition with a fifth (chloro-dicodeia-dimorphia) has also been obtained ; but, from the great similarity in properties between all the five substances and their high formulæ, it is clear that no certainty as to the purity of the missing intermediate bodies could exist, and therefore it was not thought advisable to attempt their formation.

§ 2. *Action of Hydrobromic Acid on Bromocodide.*

When bromocodide hydrobromate (prepared by two hours' digestion of codeia with three times its weight of 48 per cent. H Br, precipitation by sodium carbonate, and extraction with ether, &c.) is heated with four to six parts of the same acid to 100° for five or six hours either in a sealed tube or in an open flask, methyl bromide is copiously evolved ; the tarry

product, dissolved in warm water and precipitated by sodium carbonate, is for the most part insoluble in ether, the insoluble portion having all the properties of bromotetramorphia; the etherial extract shaken with H Cl or H Br yields a viscid liquid, which, on standing, becomes filled with crystals consisting apparently of a mixture of the hydrochlorates or hydrobromates of deoxycodeia and a lower homologue, the latter predominating when the digestion is performed in an open flask. Attempts to prevent the formation of the lower homologue by continuing the digestion with H Br for only two or three hours did not succeed, as the large quantity of unaltered bromocodide in the ether extract obtained prevents the separation of the crystalline hydrochlorate or hydrobromate of deoxycodeia, and hitherto no method of separating the deoxycodeia salt from its lower homologue has been arrived at.

The following numbers were obtained by the analysis of these crystals after recrystallization from hot water to free them from adhering bromocodide salt :—

Specimen A, prepared in sealed tubes, digested six hours at 100° :

0·3185 grm. gave 0·7850 CO₂ and 0·1970 H₂ O.

0·2200 grm. gave 0·1025 Ag Cl.

Specimen B, prepared in an open flask, digested six hours at 100°:

0·2825 grm. gave 0·6920 CO₂ and 0·1660 H₂ O.

0·2260 grm. gave 0·1085 Ag Cl.

Calculated.						Found.	
Deoxycodeia.			Deoxymorphia.			A.	B.
C ₁₈	216	67·60	C ₁₇	204	66·77	67·22	66·80
H ₂₂	22	6·89	H ₂₀	20	6·55	6·87	6·54
N.....	14	4·39	N.....	14	4·59		
O ₂	32	10·01	O ₂	32	10·47		
Cl.....	35·5	11·11	Cl.....	35·5	11·62	11·53	11·88
C ₁₈ H ₂₁ NO ₂ .HCl	319·5	100·00	C ₁₇ H ₁₉ NO ₂ .HCl	305·5	100·00		

The hydrobromate prepared from the same batch as specimen B above gave the following numbers after drying at 100° :—

0·3260 grm. gave 0·7010 CO₂ and 0·1720 H₂ O.

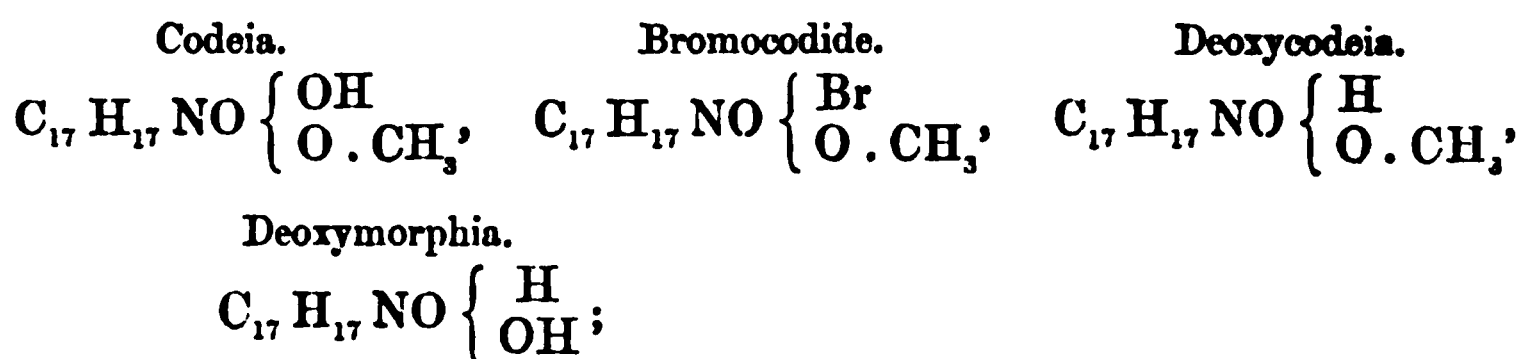
0·2730 grm. gave 0·1465 Ag Br.

Calculated.					Found.
Deoxycodeia.			Deoxymorphia.		
C ₁₅	216	59·34	C ₁₇	204	58·29
H ₂₂	22	6 05	H ₂₀	20	5·71
N.....	14	3·84	N.....	14	4·00
O ₂	32	8·79	O ₂	32	9·14
Br.....	80	21·98	Br.....	80	22·86
C ₁₅ H ₃₁ NO ₂ ,HBr	364	100·00	C ₁₇ H ₁₉ NO ₂ ,HBr	350	100·00
					58·64
					5·86
					22·83

From the above numbers, and more especially from the percentages of H, Cl, and Br found, it appears that while specimen A may have contained some little quantity of deoxycodeia, specimen B must have consisted almost wholly of the lower homologue; to this body the name deoxymorphia may appropriately be given (provisionally), to indicate that its composition bears the same relation to that of morphia as deoxycodeia to codeia.

The numbers required for apomorphia salts are very close to those actually obtained above, viz. for hydrochlorate $C=67.22$, $H=5.93$, $Cl=11.70$; and for hydrobromate $C=58.62$, $H=5.17$, $Br=22.99$; but the entire absence of emetic properties in all these specimens, as observed by Dr. Michael Foster, conclusively proves that this base could not have been present.

Further research is required before it can be decided with certainty which of the three oxygen atoms in codeia is removed in the formation of deoxycodeia; the production of deoxymorphia with simultaneous evolution of methyl bromide, however, indicates that the oxygen that links the methyl group to the rest of the codeia residue is still present in deoxycodeia and deoxymorphia, while the production of both from bromocodide renders the following formulæ probable:—



so that deoxycodeia probably bears to codeia the same relation as free hydrogen, H_2 , to water, $H \cdot OH$, or as acetic acid, $\begin{array}{c} CH_3 \\ CO \cdot OH \end{array}$ to glycollic, $\begin{array}{c} CH_2 \cdot OH \\ CO \cdot OH \end{array}$; bromocodide corresponding similarly to hydrobromic acid, HBr , or to bromacetic acid, $\begin{array}{c} CH_2 \cdot Br \\ CO \cdot OH \end{array}$.

Experiments are in progress to gain further insight into the structure of the group $C_{17}H_{17}NO$. By the action of hydriodic acid on codeia methyl is eliminated as iodide, and the elements of free hydrogen and those of HI are added on to this group; from which, as well as from the easy polymerization to form tetracodeia bases, it appears probable that some at least of the 17 carbon atoms are connected together somewhat after the fashion of ethylene or acrylic acid, which unite readily with HI , HBr , H_2 , &c. Again, the oxidizing action of $AgNO_3$ on chlorotetramorphia is accompanied by the production of CO_2 , which renders it not improbable that the third oxygen atom exists either as the group $[CH(OH)]'$ or as CO'' .

On carefully examining, side by side, the qualitative reactions of the

hydrochlorate B and those of a specimen of pure deoxycodeia-salt from codeia (without evolution of methyl bromide), not the slightest difference was discernible between the two; in their physiological actions, too, as observed by Dr. Michael Foster, the two bodies seem perfectly alike, both being utterly dissimilar from apomorphia, from which in all other respects (qualitative reactions, percentage composition, &c.) they differ either not at all or extremely little.

The portion insoluble in ether of the batch from which hydrochlorate B was obtained was treated with HCl and fractionally precipitated by strong acid several times successively; this mode of treatment was adopted rather than that with HBr, as a much larger yield is obtained thus, the hydrochlorates of chlorotetracodeia and chlorotetramorphia being much less soluble in dilute HCl than the corresponding brominated bodies are in dilute HBr. Finally, nearly white flakes were obtained presenting all the characters of chlorotetramorphia hydrochlorate, and yielding the following numbers after drying at 100°:—

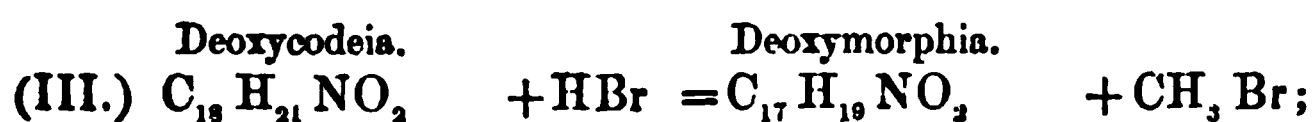
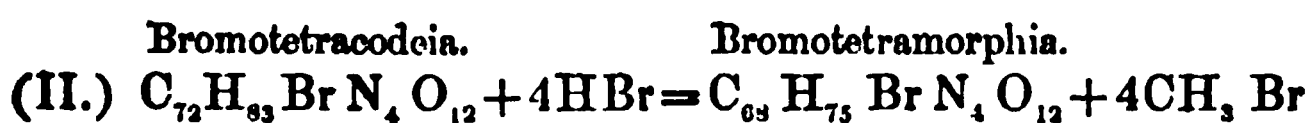
0·2480 grm. gave 0·5610 CO₂ and 0·1410 H₂ O.

0·1390 grm. gave 0·0755 AgCl.

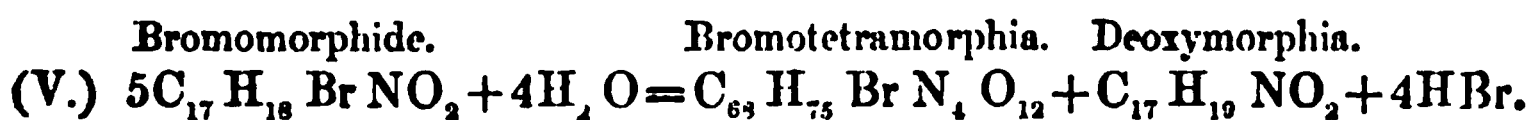
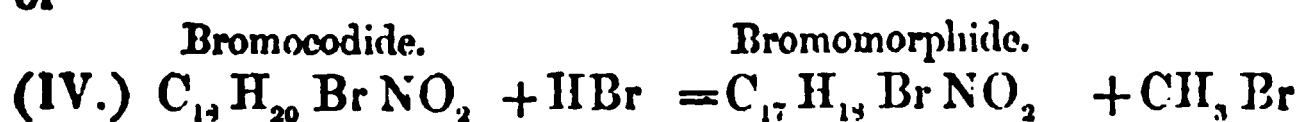
	Calculated.		Found.	
C ₆₃	816	61·79	61·70	
H ₇₉	79	5·98	6·32	
Cl ₅	177·5	13·44	13·45
N ₄	56	4·24		
O ₁₂	192	14·55		
<hr/>				
C ₆₃ H ₇₃ ClN ₄ O ₁₂ , 4HCl	1320·5	100·00		

Hence the portion insoluble in ether must have been bromotetramorphia.

The simultaneous formation of bromotetramorphia and deoxymorphia from bromocodide is explainable in two ways: either



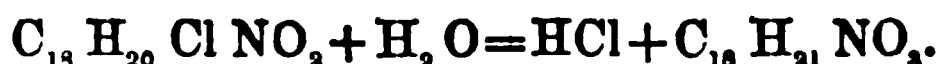
or



Of these two views, the first involves only known substances and reactions similar to those already known in the codeia series of derivatives,

and is, moreover, probable from the circumstance that the numbers obtained in some instances indicate the presence of deoxycodeia as well as deoxymorphia; whilst the second view involves the not improbable existence of bromomorphide, $C_{17}H_{18}BrNO_2$; on the other hand, it will be shown in the next section that equation (III.) represents a reaction which does not readily take place with deoxycodeia, when not in the nascent condition at any rate.

Whichever view be adopted, the ultimate formation of bromotetramorphia requires the action of water on a brominated body, substituting hydroxyl for bromine by a reaction perfectly parallel to that whereby codeia is regenerated from chlorocodide by the action of water*, viz.



§ 3. *Action of Hydrobromic Acid on Deoxycodeia.*

In the hope that this action would give rise to methyl bromide and deoxymorphia, deoxycodeia hydrobromate was heated to 100° for two hours with about five parts of 48 per cent. HBr; no change whatever took place, no methyl bromide being found on opening the tube in which the digestion was carried on after complete cooling. After an hour's additional exposure to a temperature of 120° – 130° , the contents of the tube were found to have become black and tarry, while a small quantity of methyl bromide floated on the top. Precipitated by sodium carbonate, a very dirty substance was obtained, which was almost insoluble in ether; the etherial extract, shaken with HBr, gave a small quantity of a tarry hydrobromate, of which 0.1330 grm. gave 0.0790 AgBr or Br = 25.20 per cent, deoxymorphia hydrobromate requiring only 22.86 per cent.

Nothing fit for analysis could be obtained from the portion insoluble in ether, and the minute yield of pure deoxycodeia from codeia precluded a repetition of the experiment.

§ 4. *On the Physiological Action of the foregoing Codeia derivatives.*

By MICHAEL FOSTER, M.A., M.D.

The hydrochlorate of chlorotetracodeia and the hydrobromate of bromotetramorphia, in doses of a decigramme by subcutaneous injection or by the mouth, produced in adult cats in a very few minutes a condition of great excitement, almost amounting to delirium, accompanied by a copious flow of saliva and great dilatation of the pupils. Micturation and defæcation occurred in some instances, and vomiting was observed on two occasions with the morphia-salt, but was very slight. The excitement was very peculiar, being apparently due partly to increased sensitiveness to noises, and partly to an impulse to rush about.

The same doses of the morphia-salt given to a young kitten produced the same flow of saliva, dilatation of pupils, and excitement (without vomiting);

* Matthiessen and Wright, Proc. Roy. Soc. vol. xviii. p. 88.

but the stage of excitement, which in adult cats passed gradually off in a few hours, was followed by a condition marked by a want of coordination of muscular movements, and presenting the most grotesque resemblance to certain stages of alcoholic intoxication. This stage was followed in turn by sleepiness and stupor, in which the kitten was left at night; in the morning it was found dead.

Two observations have shown that these salts paralyze (in dogs and cats) the inhibitory fibres of the pneumogastric; they also seem to lower the internal tension, but want of material has prevented me from ascertaining how this is brought about.

On *rabbits* neither salt, even in doses of a decigramme, seems to have any effect, except perhaps a slight excitement. There is no dilatation of the pupils, no flow of saliva, and, if one observation can be trusted, no paralysis of the inhibitory fibres of the pneumogastric.

No marked difference was observable between the two salts, except that the morphia-salts seemed rather more potent than the corresponding codeia bodies.

The salts of deoxycodeia and deoxymorphia given by mouth or by subcutaneous injection in doses of a decigramme, produced in adult cats, almost immediately after exhibition, a series of convulsions much more epileptic in character than tetanic. In one case there was a distinct rotatory movement.

In a few minutes these convulsions passed away, leaving the animal exhausted and frightened. Then followed a stage of excitement with dilated pupils and flow of saliva, very similar to the effects of the tetracodeia and tetramorphia salts, but less marked.

Doses of half a decigramme given to adult cats produced the stage of excitement only, without the convulsions.

In no case, with any specimen of product, has vomiting been witnessed; like the tetracodeia and tetramorphia products, the deoxycodeia and deoxymorphia salts appear to paralyze the inhibitory fibres of the pneumogastric. Trials with rabbits gave only negative results.

No marked differences could be observed between the hydrochlorates and hydrobromates of deoxycodeia or deoxymorphia.

VI. "On the Measurement of the Chemical Intensity of Total Daylight made at Catania during the Total Eclipse of Dec. 22, 1870."

By HENRY E. ROSCOE, F.R.S., and T. E. THORPE, F.R.S.E.

Received June 15, 1871.

(Abstract.)

The following communication contains the results of a series of measurements of photochemical action made at Catania in Sicily, on Dec. 22nd, 1870, during the total solar eclipse of that date, with the primary object of determining experimentally the relation existing between this action

and the changes of area in the exposed portion of the sun's disk. The attempt to establish this relation has already been made by one of us from the results of observations carried out by Captain John Herschel, R.E., F.R.S., at Jamkandi, in India, during the total eclipse of Aug. 18, 1868. Unfortunately the state of the weather at Jamkandi at the time of the eclipse was very unfavourable, and the results were therefore not of so definite a character as could be desired, and it appeared important to verify them by further observation. The method of measurement adopted is that described by one of us in the Bakerian Lecture for 1865; the observations were made in the Garden of the Benedictine Monastery of San Nicola, at Catania, the position of which, according to the determination of Mr. Schott of the United States' Coast Survey, is lat. $37^{\circ} 30' 12''$ N., long. $1^{\text{h}} 0' 18''$ E. In order to obtain data for determining the variation in chemical intensity caused by the alteration in the sun's altitude during the eclipse, observations were made on the three previous days, during which the sky was perfectly cloudless.

In the following Table the observations taken at about the same hours are grouped together:—

TABLE I.

Mean Altitude.	No. of Observations.	Chemical Intensity.		
		Diffused.	Direct.	Total.
$1^{\circ} 30' 28''$	1	0.009	0.000	0.009
9 28 10	7	0.044	0.008	0.052
13 9 57	7	0.050	0.014	0.064
19 57 49	12	0.072	0.028	0.100
24 46 12	7	0.095	0.049	0.144
28 24 10	14	0.108	0.047	0.155

The above numbers confirm the conclusion formerly arrived at, viz. that the relation between total chemical intensity and sun's altitude is represented by a straight line, or by the equation

$$CI_a = CI_0 + \text{const.} \times a,$$

where CI_a signifies the chemical intensity at any altitude a in circular measure, CI_0 the chemical intensity at 0° , and const. a a number derived from the observations.

The observations on the day of the eclipse (the 22nd) were commenced about nine o'clock A.M., and up to the time of first contact were made regularly at intervals of about an hour. The sky up to this point was cloudless, and the measurements almost absolutely coincided with the mean numbers of the preceding day's observations. As the eclipse progressed, and the temperature of the air fell, clouds were rapidly formed,

and from 1^h 40' up to the time of totality it was impossible to make any observations, as the sun was never unclouded for more than a few seconds at a time. As the illuminated portion of the solar disk gradually increased after totality, the clouds rapidly disappeared, the amount falling from 9 (overcast = 10) to 3 in about fifteen minutes. The observations were then regularly continued to within a few minutes of last contact.

Although the disk and by far the largest portion of the heavens were completely obscured by clouds during totality, rendering any determination of the photochemical action perfectly valueless for our special object, it was yet thought worth while to attempt to estimate the chemical intensity of the feebly diffused light at this time, which certainly is capable of producing photographic action.

Immediately after the supposed commencement of totality the slit was opened, and the sensitive paper exposed for ninety-five seconds. Not the slightest action, however, could be detected on the paper, and we therefore believe that we are correct in estimating the intensity of the chemically active light present at certainly not more than 0.003 of the unit which we adopt, and probably much less.

The Table containing the experimental numbers and the graphic representation of them are given in the memoir. By a graphical method the relative areas of the sun uneclipsed at the times of observation were obtained; and these are seen in column 3 of Table II., the area of the unobscured sun being taken as unity.

Column 2 gives the results of the photochemical observations made during the eclipse obtained from the graphical mean, and corrected for variation in the sun's altitude, the total chemical action immediately before first contact being taken as unity. Column 1 gives the apparent solar times of observation.

TABLE II.

1.	2.	3.
12 ^h 44'	0.915	0.961
12 54	0.876	0.880
1 16	0.686	0.637
1 24	0.555	0.534
2 2	0.000	0.000
2 9	0.165	0.127
2 25	0.307	0.338
2 34	0.464	0.498
2 44	0.601	0.602
2 54	0.725	0.736
3 4	0.876	0.861

From these observations we conclude *that the diminution in the total chemical intensity of the sun's light during an eclipse is directly proportional to the magnitude of the obscuration.*

The question of the variation of (1) the direct and (2) the diffused radiation is next discussed. On comparing the curve representing the chemical intensity of diffused light with the curve of solar obscuration, it is found that the rate of diminution in chemical action exerted by the diffused light is up to a certain point greater than corresponds to the portion of eclipsed sun, whilst from this point up to totality the rate of diminution becomes less than corresponds to the progress of the eclipse. The same rapid diminution in the chemical action of the diffused daylight during the early periods of the eclipse was also observed at Jamkandi; it is doubtless due to the dark body of the moon cutting off the light from the brightly illuminated portion of sky lying round the solar disk.

The results of the observations at Catania are then compared with those made at Moita, near Lisbon, and communicated to the Society in 1870. This comparison shows a striking coincidence between the two sets of observations. In each case it is seen that the relation between solar altitude and total chemical intensity is represented by a straight line, although the Catania observations slightly exceed, by a constant difference, those made at Moita in conformity with the slight difference in latitude, and with the fact that the former determinations were made at a greater elevation above the sea-level.

The Catania observations further confirm the fact which we formerly announced, that for altitudes below 50° the amount of chemical action effected in the plane of the horizon by diffused daylight is greater than that exerted by direct radiation, and also that at altitudes below 10° direct sunlight is almost completely robbed of its chemically active rays.

VII. "On the Calculation of Euler's Constant." By J. W. L. GLAISHER, B.A., F.R.A.S. Communicated by JAMES GLAISHER, F.R.S. Received June 6, 1871.

The main object of the present communication is to correct some inaccuracies both of reasoning and calculation contained in two papers by Mr. Shanks, viz. "On the Extension of the Value of the Base of Napier's Logarithms; of the Napierian Logarithms of 2, 3, 5, and 10; and of the Modulus of Briggs on the common System of Logarithms; all to 205 places of Decimals," in the Proc. Roy. Soc. vol. vi. p. 397; and "On the Calculation of the Numerical Value of Euler's Constant," in the Proc. Roy. Soc. vol. xv. p. 429 (1867).

For the calculation of the constant Mr. Shanks has used (as, indeed, has every calculator who has computed the value of the constant during the present century) the semiconvergent series

$$\gamma = 1 + \frac{1}{2} + \frac{1}{3} \dots + \frac{1}{x} - \log x - \frac{1}{2x} + \frac{B_1}{2x^2} - \frac{B_2}{4x^4} + \frac{B_3}{6x^6} - \dots, \quad (i)$$

γ being the constant, and B_1, B_2, B_3, \dots Bernoulli's numbers.

Mr. Shanks obtains the value of γ from this formula by making $x=10$, 20, 50, 100, 200, 500, and 1000, and remarks, as a curious coincidence, that the number of decimal places obtained from x being made equal to 10, 20, 50, and 100 is nearly proportional to $\sqrt[3]{10}$, $\sqrt[3]{20}$, $\sqrt[3]{50}$, and $\sqrt[3]{100}$. On p. 432 he gives a Table of the number of decimal places obtainable from the formula when x has the values 2, 5, 10, 20, 50, 100, 200, 500, and 1000, and from it draws the inference that "we may fairly infer that when n is increased in a geometrical ratio, the corresponding number of decimals obtained in the value of E increases only in something like an arithmetical one, and that probably from 50,000 to 100,000 terms in the Harmonic Progression would require to be summed in order to obtain 100 places of decimals in the value of E , Euler's constant."

Algebraically, of course, γ is independent of x in the formula (i), but arithmetically, since the series ultimately becomes divergent, the value of γ is so far dependent on x that for a given value of x the series will only afford a certain number of decimal places. The number of decimal places directly obtainable is equal to the number of ciphers which precede the first significant figure in the value of the numerically least term of the series.

The n th term (considering only the terms after $\frac{1}{2x}$ in (i), so that the n th term is the term involving B_n)

$$= \frac{B_n}{2n \cdot 1^{2n}},$$

which, since

$$B_n = \frac{2(1 \cdot 2 \cdot 3 \dots 2n)}{(2\pi)^{2n}} \left(1 + \frac{1}{2^{2n}} + \frac{1}{3^{2n}} + \dots \right),$$

is very nearly equal to

$$\frac{2\{1 \cdot 2 \cdot 3 \dots (2n-1)\}}{(2\pi x)^{2n}};$$

so that the ratio of the n th to the $(n-1)$ th term

$$= \frac{(2n-2)(2n-1)}{4\pi^2 x^2} = \frac{n^2}{\pi^2 x^2} \left(1 - \frac{3}{2n} \right)$$

very nearly.

Let m be the greatest integer contained in $x\pi$ so that $x\pi = m + f$, f being a proper fraction, then the ratio of the m th to the $(m-1)$ th term

$$\begin{aligned} &= \frac{m^2}{(m+f)^2} \left(1 - \frac{3}{2m} \right) \\ &= 1 - \frac{4f+3}{2m}, \end{aligned}$$

which is always less than unity.

The ratio of the $(m+1)$ th to the m th term is found in a similar manner to be

$$1 + \frac{2}{m} \left(\frac{1}{4} - f \right),$$

which is greater or less than unity according as f is less or greater than $\frac{1}{4}$.

The ratio of the $(m+2)$ th to the $(m+1)$ th term

$$= 1 + \frac{2}{m} \left(\frac{5}{4} - f \right),$$

which is always greater than unity.

Thus the m th or $(m+1)$ th term is the least according as f is less or greater than $\frac{1}{4}$.

When m is large the value of the m th term is very nearly

$$\frac{1}{m} \cdot \frac{1 \cdot 2 \cdot 3 \dots 2m}{(2\pi x)^{2m}},$$

which, by use of the theorem

$$1 \cdot 2 \cdot 3 \dots x = \sqrt{(2\pi x)} x^x e^{-x} \left(1 + \frac{1}{12x} \right),$$

becomes

$$\begin{aligned} & \frac{1}{\sqrt{m}} \cdot \frac{2 \sqrt{\pi} (2m)^{2m} e^{-2m}}{(2\pi x)^{2m}} \left(1 + \frac{1}{24m} \right) \\ &= 2 \sqrt{\frac{\pi}{m}} \cdot \left(\frac{m}{\pi e x} \right)^{2m} \left(1 + \frac{1}{24m} \right) \dots \dots \dots (ii) \end{aligned}$$

The number which forms the negative characteristic of the common logarithm of this quantity (the mantissa being made positive), reduced by unity, denotes the number of ciphers which precede the first significant figure in its value; so that if the mantissa be not made positive, the characteristic indicates the number of decimals directly obtainable from the series.

The logarithm of (ii) with its sign changed, after some expansions and reductions, becomes

$$2\mu\pi x + \frac{1}{2} \log_{10} x - \log_{10} 2 - \mu \left(\frac{f}{2x\pi} + \frac{f^2}{x\pi} + \frac{1}{24x\pi} \right),$$

μ being the modulus 43429448 Neglecting the last three terms, which can only very rarely lead to an error of a unit, we obtain as a result that the number of decimal places which the formula (i) will afford directly for a given value of x is equal to the greatest integer contained in

$$2\mu\pi x + \frac{1}{2} \log_{10} x - \log_{10} 2. \dots \dots \dots (iii)$$

The corresponding value of the least term is, of course, easily obtained from (iii), or it follows at once from (ii); for the least term

$$= 2 \sqrt{\left(\frac{\pi}{m} \right) e^{-2m} \left(\frac{m}{m+f} \right)^{2m}}$$

when m is large,

$$\begin{aligned}
 &= 2\sqrt{\left(\frac{\pi}{m}\right) e^{-2m} \left(1 + \frac{f}{m}\right)^{2m}}, \\
 &= 2\sqrt{\left(\frac{\pi}{m}\right) e^{-2m-2f}}, \\
 &= \frac{2}{\sqrt{x}} e^{-2\pi x},
 \end{aligned}$$

agreeing with (iii).

The following Table, to replace that in vol. xv. p. 432, was calculated in the way indicated above.

x .		Least term.		• Number of decimals directly obtainable from the formula.
1	3rd	2
2	7th	5
5	16th	13
10	32nd	27
20	63rd	54
50	157th	136
100	314th	273
200	629th	546
500	1571st	1365
1000	3142nd	2729

The number of decimal places practically obtainable is limited by the difficulty of calculating the Bernoulli's numbers, of which only thirty-one have been hitherto obtained. By means of these, however, 156 decimals could be obtained of γ when $x=100$; and by deducing a few of the subsequent terms, each from its predecessor (knowing their ratio), 20 places more could be obtained without difficulty.

It is clear therefore that Mr. Shanks's values of γ obtained from $x=500$ and $x=1000$ ought to agree beyond the 59th decimal, if correctly calculated. The author at first supposed that the want of agreement was due to an insufficient number of the terms involving the Bernoulli's numbers having been included, and he therefore undertook the calculation of this portion of the expression for $x=500$ and $x=1000$ to 100 decimal places; the results, however, still showed a difference in the 59th place.

In order to determine where the error existed, the same portion of the constant was calculated also to 100 places, both from $x=100$ and $x=200$; all the calculations were performed wholly in duplicate, and so much care was taken that the author felt a strong conviction of their accuracy. It should be noticed that the agreement of all the four results would not necessarily prove the accuracy of that value of γ ; for any error made in the calculation of the harmonic series $1 + \frac{1}{2} + \dots + \frac{1}{100}$ would merely pro-

duce the same error in $1 + \frac{1}{2} \dots + \frac{1}{200}$, $1 + \frac{1}{2} \dots + \frac{1}{500}$, and $1 + \frac{1}{2} \dots + \frac{1}{1000}$, supposing the calculation of the reciprocals beyond $\frac{1}{100}$ to have been accurately performed.

It was therefore evident that the only means of ensuring freedom from error in the harmonic series was to recalculate it. The lowest value of x which would suffice for a verification was 100; the author therefore calculated the value of the series $1 + \frac{1}{2} \dots + \frac{1}{100}$ to 100 places of decimals, and the result was found to agree to that extent with that given by Mr. Shanks in the paper previously referred to; the value of $1 + \frac{1}{2} \dots + \frac{1}{50}$ was also found to be correct.

It may be mentioned that the calculation was abbreviated by a simple artifice, suggested by Oettinger (*Crelle's Journal*, t. lx. p. 376), which will be easily understood from an example. Suppose the sums of the reciprocals of the odd and even numbers up to 50 are known, and it is required to find the sum of the reciprocals up to 100.

Let

$$\alpha = 1 + \frac{1}{3} + \frac{1}{5} \dots \frac{1}{49},$$

$$\beta = \frac{1}{2} + \frac{1}{4} + \frac{1}{6} \dots \frac{1}{50};$$

then

$$\frac{\alpha + \beta}{2} = \frac{1}{2} + \frac{1}{4} + \frac{1}{6} \dots + \frac{1}{98} + \frac{1}{100};$$

so that

$$\begin{aligned} & 1 + \frac{1}{2} + \frac{1}{3} \dots + \frac{1}{99} + \frac{1}{100} \\ &= \frac{\alpha + \beta}{2} + \alpha + \frac{1}{51} + \frac{1}{53} \dots + \frac{1}{99}, \end{aligned}$$

and the calculation of the reciprocals of the even numbers between 50 and 100 is rendered unnecessary.

The values of $\log 2$ and $\log 5$, which were required to form $\log 100$, $\log 200$, $\log 500$, and $\log 1000$ were taken from Mr. Shanks's '*Rectification of the Circle*' (they are also given in the *Proc. Roy. Soc.* vol. vi. p. 397), and the summations of the harmonic series from Mr. Shanks's paper in vol. xv. p. 431 of the *Proc. Roy. Soc.* The results were as follows:—

From $x = 100$:—

$\gamma =$	57721	50649	01532	80000	65120
	90082	40243	10421	59335	93092
	35988	05770	42799	90853	75858
	22362	13134	84454	84202	91195 ... (A).

From $x = 200$:—

$\gamma = \cdot 57721$	56649	01532	86060	65120
90082	40243	10421	59335	93995
35988	05770	91390	77706	70283
52607	02838	27101	38332	64367 ... (B).

From $x = 500$:—

$\gamma = \cdot 57721$	56649	01532	86060	65120
90082	40243	10421	59335	93995
35988	05771	53864	75089	05973
20064	75314	10504	07812	29873 ... (C).

From $x = 1000$:—

$\gamma = \cdot 57721$	56649	01532	86060	65120
90082	40243	10421	59335	93995
35988	05772	02455	61942	00398
50309	65017	53150	61852	03044 ... (D).

The terms involving B_{39} , B_{31} , B_{22} , and B_{19} were the highest used in the calculation of (A), (B), (C), and (D) respectively.

It will be seen that (A), (B), (C), and (D) differ in two respects: (i) the 50th figure in (A) is 2, while in (B), (C), and (D) it is 5, and (ii) all four values are totally different after the 59th figure.

The first discrepancy (i) pointed to an error in the summation of the harmonic series. As the author had verified Mr. Shanks's value of

$1 + \frac{1}{2} \dots + \frac{1}{100}$, it was practically certain that (A) was the correct value.

To place this beyond all doubt, however, the author calculated γ from $x = 50$ to 57 places of decimals, and the result entirely confirmed (A). It follows, therefore, that Mr. Shanks has made an error of 3 in the 50th

place in the calculation $\frac{1}{100} + \frac{1}{101} \dots + \frac{1}{200}$, so that we have the fol-

lowing errata (vol. xv. p. 431) :—In $1 + \frac{1}{2} \dots + \frac{1}{200}$, the tenth group of five should be ... 53024 ... instead of ... 53027 ...

In $1 + \frac{1}{2} \dots + \frac{1}{500}$ the tenth group should be ... 39677 ... instead of ... 39680 ...

In $1 + \frac{1}{2} \dots + \frac{1}{1000}$ the tenth group should be ... 70880 ... instead of ... 70883 ...

With regard to the second error (ii), which causes the disagreement of all the figures after the 59th, it is clear that no error in the Bernoulli's numbers could produce such a discrepancy, as the terms involving the latter vary their position in each calculation, so that an error in any one of them could not affect the same decimal in (A), (B), (C), and (D).

By subtraction we find :

(B) - (A) = ... (60 ciphers) . 48500 86852
 94425 30244 89703 42646
 54039 73172 (E)

(D) - (C) = ... (60 ciphers) . . . 48500 86852
 94425 30244 89703 42646
 54039 73171 (E)

which only differ by a unit in the 100th decimal place.
Leaving out of consideration the terms involving Bernoulli's numbers, the difference between the series when $x=200$ and when $x=100$ is

$$\left(1 + \frac{1}{2} \cdots + \frac{1}{200} - \log 200\right) - \left(1 + \frac{1}{2} \cdots + \frac{1}{100} - \log 100\right)$$
$$= \frac{1}{101} + \frac{1}{102} \cdots + \frac{1}{200} - \log 2.$$

Similarly, the difference between the series when $x=1000$ and when $x=500$ is

$$= \frac{1}{501} + \frac{1}{502} \cdots + \frac{1}{1000} - \log 2.$$

Now as it is extremely improbable that two errors, exactly equal in amount, should have been made in the calculations of

$$\frac{1}{101} + \frac{1}{102} \cdots + \frac{1}{200} \text{ and } \frac{1}{501} + \frac{1}{502} \cdots + \frac{1}{1000},$$

we have very strong evidence that the value of $\log 2$ is inaccurate, and that (E) is the correction to be applied to it.

By subtracting (C) from (A), we obtain

(C) - (A) = ... (50 ciphers) . . . 1 11064 84235
 30114 97702 62179 26049 23519
 38678 (F)

The difference of the corresponding series (omitting as before the terms involving the Bernoulli's numbers)

$$= \frac{1}{101} + \frac{1}{102} \cdots + \frac{1}{500} - \log 5.$$

Having only this one* difference-result involving $\log 5$, it is impossible to decide from (A), (B), (C), and (D) whether the harmonic series or $\log 5$ or both are in error ; but the following reasoning places it beyond all doubt that (F) is a correction to $\log 5$, and that the sum of the harmonic series is correct.

* By subtracting (B) from (D) we might get another, but the portion $\frac{1}{201} + \frac{1}{202} + \frac{1}{500}$ of the harmonic series, as well as $\log 5$, would be common to both. —June 16.

Mr. Shanks computed log 2 and log 5 from formulæ of the form

$$\begin{aligned}\text{Log } 2 &= 2 (7 P + 5 Q + 3 R), \\ \text{Log } 5 &= 2 (16 P + 12 Q + 7 R),\end{aligned}$$

P, Q, and R being infinite series (Proc. Roy. Soc. vol. vi. p. 397; 'Rectification of the Circle,' p. 88).

If, therefore, any error was made in the calculation of P say, it would produce errors in log 2 and log 5 proportional to 7 and 16. On trial it was found that sixteen times (E) was equal to seven times (F), the difference being only such as an error of a unit in the 100th decimal of (E) or (F) would produce. This afforded a moral proof that Mr. Shanks had made an error equal to one-fourteenth of (E) in the calculation of P, or

$$\frac{1}{31} + \frac{1}{3 \cdot 31^3} + \frac{1}{5 \cdot 31^5} + \dots$$

which has rendered his values of log 2 and log 5 incorrect, and that (with the exception of the error previously noticed) the harmonic series was summed correctly.

Since log 3 was calculated from the formula

$$\text{Log } 3 = 2 (11 P + 8 Q + 5 R),$$

its value is also incorrect, as also is that of log 10 (log 2 + log 5) and the modulus (the reciprocal of log 10).

The values of all these quantities are given to 205 decimal places in vol. vi. Proceedings of the Royal Society, p. 397; but all the figures after the 59th decimal place are incorrect in each case.

The correct values to 100 places are:—

Log 2=	·60314	71805	59045	30941	72321
	21458	17656	80755	00134	86025
	52541	20680	00949	33936	21969
	60471	56058	63326	99641	86875 . . .
Log 3=	1·09861	22886	68109	09139	52452
	36922	52570	46474	90557	82274
	94517	34694	33363	74942	93218
	60896	68736	15754	81373	20888 . . .
Log 5=	1·60943	79124	34100	37460	07593
	33226	18763	95256	01354	26851
	77219	12647	89147	41789	87707
	65776	46301	33878	09317	96108 . . .
Log 10=	2·30258	50929	94045	68401	79914
	54684	36420	76011	01488	62877
	29760	33327	90096	75726	09677
	35248	02359	97205	06959	82982 . . .

It is to be observed that the above value of log 3 is not quite as well determined as the others; the calculations in regard to Euler's constant

form a real verification of $\log 2$, $\log 5$, and $\log 10$; they also verify P, Q, and R; but an error in $\log 3$ in the transcription of P, Q, and R, or their multiplication by 11, 8, and 5, or in the final addition and multiplication by 2, would not be detected.

In the above logarithms the last figure may be in error to the extent of one or two units.

The value of Euler's constant to 100 decimal places is—

$\gamma = .57721$	56649	01532	86060	65120
90082	40243	10421	59335	93992
35988	05767	23488	48677	26777
66467	09369	47063	29174	67497 . . .

The last figure here also may be in error to the extent of one or two units.

It will be observed that Mr. Shanks's value* of γ for $x=500$ differs from (C) in the 65th decimal place, and that his value for $x=1000$ differs from (D) in the 73rd place. This is caused by an inaccurate value of B_{13} , having been made use of. The correct value of B_{13} is $\frac{8553103}{6}$; but Euler,

who first calculated it, made it $\frac{8553103}{2}$ ('Acta Petropolitana' for 1781, p. 46), and this incorrect value is given in the 'Penny Cyclopædia' (Article "Numbers of Bernoulli") and probably in other places.

The values of the first thirty-one Bernoulli's numbers are given in a paper by Ohm (Crelle's Journal, t. xx. p. 11), and B_{13} is given correctly there. The agreement of the values of Euler's constant contained in this paper (when the logarithms of 2 and 5 are corrected) afford a complete verification of the Bernoulli's numbers as far as B_{26} , and partial verifications of the rest.

The difficulty and inconvenience of making calculations to so many decimal places is sufficient to warrant the publication of the values of the positive and negative parts of the portion of the series involving the Bernoulli's numbers, in case any one should desire to repeat any part of the calculation. We have

$$\gamma = 1 + \frac{1}{2} + \frac{1}{3} \cdots + \frac{1}{x} - \log x - \frac{1}{2x} + \frac{B_1}{2x^2} - \frac{B_2}{4x^4} + \frac{B_3}{6x^6} - \cdots;$$

and if m denote the sum of the terms of the same sign as the harmonic series, and n the sum of the terms of the same sign as the logarithms, viz. if

$$m = \frac{B_1}{2x_2} + \frac{B_3}{6x^6} + \frac{B_5}{10x^{10}} + \cdots$$

$$n = \frac{1}{2x} + \frac{B_2}{4x^4} + \frac{B_4}{8x_8} + \cdots;$$

* Proc. Roy. Soc. vol. xv. p. 432.

then, when $x=100$:—

$m=00000$	83333	33337	30158	73773
44885	67821	37321	67823	08773
00082	30339	33761	49846	30254
14224	82020	15080	52018	00966
589 . . .				
$n=00500$	00000	83333	33375	00000
21002	80052	53907	77630	50871
21105	34750	25738	21912	47054
82311	34887	61384	97230	71640
133 . . .				

When $x=200$:—

$m=00000$	20833	33333	30533	73016
61283	50538	50711	91078	12258
22890	92137	92320	17052	51095
23400	80804	78957	35141	00774
044 . . .				
$n=00250$	00000	03208	33333	00000
37505	14900	84887	28877	00510
05685	64006	82170	11770	13023
01074	00000	38458	75084	75675
042 . . .				

When $x=500$:—

$m=00000$	03333	33333	33358	73015
87300	34487	73402	42678	21147
87464	83345	53780	01875	16204
50700	20761	22352	44796	75800
196 . . .				
$n=00100$	00000	00133	33333	33344
00000	00008	63900	92825	14227
00903	41363	50645	63522	00215
12108	84046	96404	01971	02278
386 . . .				

When $x=1000$:—

$m=00000$	00833	33333	33333	73015
87301	50487	73448	77428	21067
82137	32165	00876	22580	90464
52882	37182	41954	63031	48001
922 . . .				
$n=00050$	00000	00008	33333	33333
37500	00000	00210	92796	09823
93526	00040	82093	23718	92820
00015	30332	37713	49061	83360
485 . . .				

The calculation was performed to 105 places, and the last two figures have been rejected.

POSTSCRIPT.

Received June 14, 1871.

After the completion of the above paper, the author found that Mr. Shanks had, in second, third, and fourth supplementary papers on the Constant*, extended his calculations so as to determine γ from $x=2000$, 5000, and 10,000.

The values so obtained all differ in the sixtieth decimal; in fact the higher x is taken, the further from the truth are the results, as the errors in the logarithms are multiplied by larger factors.

The calculation for $x=2000$ affords a verification of the error of $\log 2$; for on subtracting the value of γ ($x=1000$) from γ ($x=2000$), we obtain (after correcting B_{13}) a result agreeing with E to 80 decimal places (which is as far as Mr. Shanks has calculated the latter value of γ), with the exception of a difference of a unit in the 73rd figure—an error probably in the summation of the harmonic series from 1000 to 2000.

The values for $x=5000$ and $x=10,000$ are (besides the errors previously noticed) inaccurate from the 62nd figure.

VIII. "Records of the Magnetic Observations at the Kew Observatory.

No. IV.—Analysis of the principal Disturbances shown by the Horizontal and Vertical Force Magnetometers of the Kew Observatory from 1859 to 1864." By General Sir EDWARD SABINE, K.C.B., President. Received June 15, 1871.

(Abstract.)

This paper exhibits an analysis of the principal disturbances recorded by the horizontal and vertical force self-recording magnetometers of the Kew Observatory in the years 1859 to 1864, showing the progressive diminution in the number and value of the disturbances from a maximum in 1859 to a minimum in 1863, being the first moiety of the "decennial period;" and showing also the distribution of the disturbances, increasing or diminishing the respective forces, in the several years, months, and hours.

IX. "Amended Rule for working out Sumner's Method of finding a Ship's Place." By Prof. Sir WILLIAM THOMSON, F.R.S. Received June 15, 1871.

In my previous communication on this subject (*antea*, p. 259) I described a plan according to which, in the first place, two auxiliary lines were to be drawn on the chart, from two sets of numbers taken out of a proposed Table, and then Sumner's line (the line on which the observation shows the ship to be) was to be interpolated, dividing the space between them in the proportion of the differences of the sun's decli-

* Proc. Roy. Soc. vol. xvi. pp. 154, 299, vol. xviii. p. 49.

nation from two of the tabular numbers. I find a better plan in practice to be as follows:—

(1) Take two solutions out of the Table as directed in my previous paper.

(2) Taking the two hour-angles and the two altitudes from these two solutions, interpolate to the nearest minute the hour-angle and the altitude corresponding to the correct declination, according to the simple proportion of its differences from the declinations of the two solutions; and estimate, by inspection, the proper azimuth to the nearest half degree, from the azimuths shown in the two solutions.

(3) Using the interpolated hour-angle, azimuth, and altitude found by clause (2), find on the chart, in the assumed parallel of latitude, the point whose longitude is the difference between the interpolated hour-angle and the Greenwich hour-angle at the time of the observation; through this point draw, by aid of a protractor, a line inclined to the north and south at an angle equal to the azimuth, and on the proper side according to whether the observation was made before or after noon; on this azimuthal line* measure off towards the sun a length (miles for minutes) equal to the correct altitude of observation above the interpolated altitude of clause (2); and through the point thus reached draw a perpendicular to the azimuthal line. This perpendicular is Sumner's line.

The Table (of which a specimen page was shown in my former communication) has now been completed by Mr. Roberts, and has been in my hands long enough to allow me to test its use in actual practice. I find the assistance of compasses for measuring off the assumed colatitude preferable to the slip of card with numbers which I first suggested; and I find the process to be altogether very easy and *unfatiguing* (in respect to fatigue a great contrast to the ordinary method). I find that all the cases (as azimuth and hour-angle both acute, azimuth acute and hour-angle obtuse, or azimuth and hour-angle both obtuse, or, again, declination greater than latitude, but of same name, and declination of opposite name to latitude) work out without ambiguity or perplexity. Still the mere fact of there being different cases may possibly deter practical navigators from leaving the ordinary method, which, though considerably longer and *much more laborious*, has the excellent quality of presenting no variety of cases. I intend, however, to push forward the preparation of a short paper of practical directions, illustrated by examples of all ordinary and critical cases, and to publish it with the Table; so that practical men may have an opportunity of judging from actual experience whether the plan of working Sumner's method which I have proposed will be useful to them or not.

I thought it unnecessary in my former communication to remark that every determination of longitude at sea (except from soundings or sights

* It is unnecessary to mark this azimuthal on the chart. By holding one side of a "set square" (or other proper drawing instrument for making right angles) along the azimuthal line, the Sumner line perpendicular to it is readily drawn, and this "Sumner line," or line of equal altitude, is the only mark which need be actually made on the paper.

of land interpreted in connexion with observations for latitude) involves the unknown error of the chronometer, and makes the ship 1' West or East of the true place for every four seconds of time that the chronometer's indication is in advance of or behind correct Greenwich time. Although I believe that every man who uses a chronometer at sea knows this perfectly well, I shall not omit to state it in the practical directions which I propose to publish, as the Astronomer Royal, Professor Stokes ('Proceedings,' April 27, 1871), and Mr. Gordon (writing in the 'Mercantile and Shipping Gazette') are of opinion that an explicit warning of the kind might be desirable in connexion with any publication tending to bring Sumner's method into more general use than it has been hitherto.

X. "On Linear Differential Equations."—No. V. By W. H.

L. RUSSELL, F.R.S. Received June 15, 1871.

Let us now endeavour to ascertain under what circumstance a linear differential equation admits a solution of the form $P \log_e Q$, where P and Q are rational functions of (x) .

$$\text{If } (a_0 + a_1x + \dots) \frac{d^n y}{dx^n} + (\beta_0 + \beta_1x + \dots) \frac{d^{n-1}y}{dx^{n-1}} + \dots = 0,$$

we have, substituting $y = P \log_e Q$,

$$\left\{ (a_0 + a_1x + \dots) \frac{d^n P}{dx^n} + (\beta_0 + \beta_1x + \dots) \frac{d^{n-1}P}{dx^{n-1}} + \dots \right\} \log_e Q + R = 0,$$

where R is a rational function of (x) . Hence

$$(a_0 + a_1x + \dots) \frac{d^n P}{dx^n} + (\beta_0 + \beta_1x + \dots) \frac{d^{n-1}P}{dx^{n-1}} + \dots = 0,$$

or P must be a rational function satisfying the given equation. Having ascertained its value, we have a differential equation of the form

$$L_0 \frac{d^n \log_e Q}{dx^n} + L_1 \frac{d^{n-1} \log_e Q}{dx^{n-1}} + \dots + L_n \log_e Q = 0.$$

Divide this equation by L_n and differentiate, and we have an equation of the form

$$M_0 \frac{d^{n+1} \log_e Q}{dx^{n+1}} + M_1 \frac{d^n \log_e Q}{dx^n} + \dots + M_n \frac{d \log_e Q}{dx} = 0;$$

from which we find Q in all possible cases, since $\frac{d \log_e Q}{dx}$ is a rational function of (x) .

It is impossible that a linear differential equation can in general have a solution of the form $y = f(\log_e x)$; for in that case we should have

$$(a_0 + a_1x + a_2x^2 + \dots) \frac{d^n f(\log_e x)}{dx^n} + (\beta_0 + \beta_1x + \beta_2x^2 + \dots) \frac{d^{n-1} f(\log_e x)}{dx^{n-1}} + \dots = 0.$$

Let $z = \log_e x$, and the equation becomes of the form

$$(a_0 + a_1e^z + a_2e^{2z} + \dots) \frac{d^n f(z)}{dz^n} + (b_0 + b_1e^z + b_2e^{2z} + \dots) \frac{d^{n-1} f(z)}{dz^{n-1}} + \dots = 0;$$

and putting for z successively $z + 2\pi i$, $z + 4\pi i$, . . . , the equation becomes

$$(a_0 + a_1 e^z + a_2 e^{2z} + \dots) \frac{d^n f(z + 2\pi i)}{dz^n} + (b_0 + b_1 e^z + b_2 e^{2z} + \dots) \frac{d^{n-1} f(z + 4\pi i)}{dz^{n-1}} + \dots = 0,$$

$$(a_0 + a_1 e^z + a_2 e^{2z} + \dots) \frac{d^n f(z + 4\pi i)}{dz^n} + (b_0 + b_1 e^z + b_2 e^{2z} + \dots)$$

$$\frac{d^{n-1} f(z + 4\pi i)}{dz^{n-1}} = 0,$$

$$\&c. = \&c.,$$

where these equations can be indefinitely continued.

Let us now see what are the conditions that a linear differential equation can admit of a solution $y = P + \sqrt[n]{Q}$, where P and Q are rational functions of (x) . It is evident that P and Q must satisfy the differential equation separately, so that we may confine ourselves to the case of $y = \sqrt[n]{Q}$.

We observe that the factors of Q must also be factors of the coefficient of the highest differential; i. e. if

$$a_0 + a_1 x + a_2 x^2 + \dots = (x-a)^m (x-b)^r \dots \sqrt[n]{Q} = (x-a)^\mu (x-b)^\nu \dots$$

Let $x = a + z$ in the differential equation, and expand y in ascending powers of (z) . We have then an equation to determine μ , and ν . . . may be found in the same way. Let

$$(x+2)(x^2-1) \frac{d^2 y}{dx^2} + (x^3 + 2x^2 + 2x - 2) \frac{dy}{dx} - xy = 0.$$

Let $z = x + 2$, and the equation becomes

$$z(z^2 - 4z + 3) \frac{d^2 y}{dz^2} + (z^3 - 4z^2 + 6z - 6) \frac{dy}{dz} - (z-2)y = 0.$$

Let $y = Az^n + Bz^{n+1} + \dots$, which gives $3n(n-1) - 6n = 0$, whence $n = 0$, or 3.

Let $x = z + 1$:

$$z(z+2)(z+3) \frac{d^2 y}{dz^2} + (z^3 + 5z^2 + 9z + 3) \frac{dy}{dz} - (z+1)y = 0.$$

Here, putting y as before, we have $6n(n-1) + 3n = 0$, whence $n = 0$, or $\frac{1}{2}$. Lastly, let $x = z - 1$:

$$z(z+1)(z-2) \frac{d^2 y}{dz^2} + (z^3 - z^2 + z - 3) \frac{dy}{dz} - (z-1)y = 0.$$

Here $2n(n-1) + 3n = 0$, or $n = 0$ or $-\frac{1}{2}$. Hence the possible forms are

$$\sqrt{x-1}, \quad \frac{1}{\sqrt{x+1}}, \quad \text{and} \quad \frac{\sqrt{x-1}^*}{\sqrt{x+1}}$$

the last of which succeeds.

* And also $(x+2)^3 \sqrt{x-1}$, $\frac{(x+2)^3}{\sqrt{x+1}}$, $\frac{(x+2)^3 \sqrt{x-1}}{\sqrt{x+1}}$.—W. H. L. R.—June 30.

I have chosen a particular case, but it is manifest that the equation

$$(a + \beta x + \gamma x^2 + \zeta x^3) \frac{d^2 y}{dx^2} + (a' + \beta' x + \gamma' x^2 + \zeta' x^3) \frac{dy}{dx} + (a'' + \beta'' x + \gamma'' x^2 + \zeta'' x^3) y = 0$$

could be treated in the same manner. There will be eight possible forms of solution of the class we have here considered, but in practice the number of trials will be much reduced if we do not consider the incommensurable roots of (n).

XI. "On the Undercurrent Theory of the Ocean, as propounded by recent explorers." By Captain SPRATT, C.B., R.N., F.R.S.
Received June 15, 1871.

The universal undercurrent theory so fascinatingly advocated by Maury and others, and more recently by the late Dr. Forchhammer, has now been so remarkably supported and maintained in the enlarged views pronounced by Dr. Carpenter in his recent papers and lectures before the Royal and other societies, and is of so interesting and important a nature in connexion with the study of the laws regulating the natural history and geological results of the past, as well as of the phenomena in progress in the ocean and seas in communication with it, that the assumed facts and data upon which they are based deserve, and indeed require, in the interest of sound science and philosophy, to be carefully considered and analyzed before they can be accepted as a grand law such as is implied in the views or theory.

Dr. Carpenter has put forward certain axioms as "propositions" or fundamental principles, as necessary results from the influence of rivers and rain, temperature, evaporation, and density upon the surface and deeps of all seas that are in communication. I feel it necessary to give the more important of these, as being the basis of his theory*.

"No. III. That wherever there is a *want of equilibrium* arising from *difference of density* between two columns of water in communication with each other, there will be a tendency towards the restoration of equilibrium by a flow from the *lowest stratum* of the denser column towards that of the lighter, in virtue of the excess of pressure to which the former is subjected.

"No. IV. That so long as the like difference of density is maintained, so long will this flow continue; and thus any agency which permanently disturbs the equilibrium in the same sense, either by increasing the density of one column, or by diminishing that of the other, will keep up a permanent flow from the lower stratum of the denser towards that of the less dense. This constant *tendency to restoration of equilibrium* will keep the actual difference of density within definite limits.

"No. V. That if there be at the same time a *difference of level* and an

* See *antè*, p. 211.

excess of density on the side of the shorter column, there will be a tendency to the restoration of the *level* by a *surface-flow from the higher to the lower*, and a tendency to the restoration of the *equilibrium* by an *under-flow* in the opposite direction *from the heavier to the lighter column*."

Dr. Carpenter then refers to the observations and opinions* of the late Dr. Forchhammer of Copenhagen, for the conditions and facts that are said or assumed to exist between the Baltic and German Ocean in regard to the interchange of a surface and undercurrent between them so as to restore the lost density of the former caused by a supposed continued outward surface-current, and thus maintain the equilibrium or normal conditions between them.

As I shall be obliged to refer to these opinions as assumed facts hereafter, I must here dwell more particularly upon what has been assumed in regard to the Black Sea and Ægean, regarding which Dr. Carpenter states as follows:—"The condition of the Euxine is precisely parallel to that of the Baltic; and a surface-current is well known to be constantly flowing outwards through the Bosphorus and Dardanelles, carrying with it (as in the case of the Baltic) a large quantity of salt. Now, as the enormous volume of fresh water discharged into the Euxine by the Danube, the Dneiper, and the Don would in time wash the whole of the salt out of its basin, it is obvious that its density can only be maintained at its constant amount (about two-fifths that of ordinary sea-water) by a continual inflow of denser water from the Ægean, the existence of which inflow, therefore, may be predicted on the double ground of *à priori* and *à posteriori* necessity."

The first consideration then is, whether these predictions, as necessary premises to his enlarged theory, are true both for the Baltic and Black seas, since upon them the soundness of the theory mainly depends.

I am therefore induced, as I have been frequently requested to do, to give my experience and knowledge of the conditions of the surface and depths of the Mediterranean and Black Sea, and next analyze the observations of Dr. Forchhammer to see whether his opinions and conclusions are true in regard to the Baltic, viz. that the density of the Baltic is maintained by an undercurrent from the denser waters of the Kattegat and German Ocean. For it is through my knowledge and experience of the conditions existing at the Black Sea Straits and Ægean Sea, from repeated experiments in them on temperature and densities, &c., that, with all due deference to the judgment of Dr. Carpenter, I am obliged to regard those advanced by him as fallacious, and to maintain that, in adopting them, he is advocating a theory upon erroneous premises. It is therefore necessary for me to briefly show here the nature and extent of these investigations, either published or unpublished, in support of my reasons for so differing from Dr. Carpenter.

* Philosophical Transactions, 1865.

In August 1848 I read a paper at the Swansea Meeting of the British Association*, "On the Influence of Temperature upon the Distribution of the Fauna in the Ægean Sea," as an explanation of the zones of animal life in that sea, which had been discovered by Edward Forbes a few years previously, when we were employed together in the 'Beacon.' I then showed that the temperature in depths below about 100 fathoms, although sometimes in midsummer trenching down to between 200 and 300 fathoms, was always uniform, the minimum of $55\frac{1}{2}^{\circ}$ Fahr. (as the thermometers then gave) being reached at that depth; and this fact I had discovered in 1845 to exist in both divisions of the Mediterranean, although in the latter the minimum was not so low as in the Ægean by about 3° or $3\frac{1}{2}^{\circ}$. As the thermometers in use at that time were defective in construction for such observations by (as now found) a constant error in excess of about $3\frac{1}{2}^{\circ}$ or 4° , the temperatures, with this deduction made from them, agree with the recent observations of Dr. Carpenter. Also, in a paper published in the 'Nautical Magazine' for January 1862, "On the proper Depths for Deep-sea Cables," as the result of my experience gained after conducting the laying of the Varna and Crimean Cables across the Black Sea, and others in the Levant, and also between Malta and Alexandria, when the temperatures were constantly taken, I stated the following interesting facts.

Extract from the 'Nautical Magazine,' January 1862.

"The Mediterranean temperatures are known to be not very low at great depths, but reach their minimum as a permanency in from 100 to 300 fathoms; and this minimum temperature seems to correspond with the average annual temperature of the locality itself. And as the Mediterranean is divided into a series of basins, with comparatively intermediate shallows, it is its surface waters, about the depth of 200 or 300 fathoms (being that of the barriers which separate them), that unite by their superficial and encircling currents. Thus, as the depth across the Strait of Gibraltar is under 200 fathoms, the very cold waters in the deeps of the

* British Association Report, 1848 (Swansea Meeting), Sections, p. 81. Also, see Edinburgh Philosophical Journal, 1848, in which are the following remarks:—

"It is temperature, and local conditions partially arising from it, that limits the elevation and existence of animal life. So does the same law appear applicable to marine animals, which breathe the medium they inhabit.

"As a law resulting from this influence, characteristic, tropical, and subtropical species will have a limited distribution in geographical space, whilst the boreal and subboreal characters will be found in every geographic position, where corresponding regions of depth are found with animal life existing, the limit of which I believe to be much lower than 300 fathoms, having examples from 390 fathoms. But I must notice that the Ægean deep dredges indicate generally a zero of animal life at 300 fathoms, as Professor Forbes was induced to assume. I believe, however, that in the deserts of yellow clay an occasional oasis of animal life may be found in much greater depths, dependent upon some favourable local condition or accident."

Atlantic, or of the Black Sea, do not intermingle, and exert their individual temperature in the depths of the central basins. The temperature of the deeper waters of the Mediterranean, Archipelago, Sea of Marmora, and Black Sea are consequently each dependent on local influences, namely from the solar or atmospheric temperature above them. Therefore the minimum temperature of their deeper parts corresponds nearly with the mean annual temperature over them.

“In the Grecian Archipelago I long since showed it to be constant at about 55° in depths from 100 fathoms downwards. In that sea the temperature of the intermediate depths between 100 fathoms and the surface in the summer season ranges from 55° to 76° , and, indeed, even up to 80° and 86° sometimes, in the littoral waters of enclosed gulfs and shallow bays.

“In the eastern and western basins of the Mediterranean it will have consequently a higher minimum temperature than that; and I find that it is about 59° in all depths from 300 down to 2000 fathoms. But between the depths of 30 and of 300 fathoms there is an increasing variation from that temperature to 73° and to 75° in the summer months, but confined more particularly to the depths between 100 fathoms and the surface.

“But in the winter months of December, January, February, and March, the upper depth is nearly at the minimum temperature of the deepest part below, namely from 59° to 62° , varying with the locality and *depths* of water there.

“Thus it is that in these months the surface and deep waters of the Mediterranean are at a constant temperature of about 10° or 15° above that of the atmosphere.

“After the month of March, however, the solar influence begins sensibly to raise both sea and atmospheric temperatures, so that in July, in the southern part of the Mediterranean, it is at its maximum of about 75° from the surface down to the depth of about 30 fathoms.”

Having been thus brief in stating the facts obtained in regard to the distribution of the temperatures of the deeps, I shall also be as brief as will be consistent with the due illustration of the more important facts and results in describing the observations for ascertaining these surface- and under-currents in one or two of the localities in question, viz. the Dardanelles and Bosphorus, where Dr. Carpenter has assumed and predicted conditions as an absolute necessity, and upon which predictions he has mainly founded his enlarged views and theories.

Now it will easily be realized on consideration, that the testing of surface-currents for their various rates in different depths, or of under-currents of small amount where they exist, in proof of this theory, as a general fact, is an experiment that requires much delicacy and nicety in the mode of operation and the means by which it is attempted or effected. I therefore never attempted such experiments by the use of any bulky object, such as a boat, that offered both great resistance to the surface-

current, and that was also easily affected by the resistance it offered to the wind and swell, and consequently the counter drift resulting from them, and thus necessarily tending to mask the more delicate movements in the deeps and to vitiate the results.

I felt, too, that a fixed object as a point of reference was always necessary, such as a buoy or float attached to a sinker actually upon the bottom.

Such observations for testing ocean-currents, then, should only be made in connexion with a *fixed object attached to the bottom*, whether in 2000 fathoms or 20 fathoms, as I have before recommended both in the 'Nautical Magazine' and in the Appendix to 'Researches in Crete,' when treating on the question of undercurrents in the Dardanelles and *Ægean*, &c., from which latter work I shall have to quote a few passages in proof of the actual conditions existing there. I am therefore induced here to express my regret that the same means were not adopted in the Straits of Gibraltar, especially as two or three good opportunities offered when the dredge had got irrecoverably entangled at the bottom.

I now give the following extracts referring to the observations made in the *Ægean* Sea, Dardanelles, Black Sea, &c., to test the rate and depth of the current generally flowing from the latter into the former, as also the different densities of these seas and straits; and the results arrived at will be seen to have a very interesting and important bearing upon the theory and question at issue.

The densities were tested by an hydrometer, which, having a range from about $13\frac{1}{2}^{\circ}$ as the normal condition of the surface of the Black Sea, to 29° as the normal of the *Ægean* and Mediterranean seas, served to show the varying densities in different depths and localities between them with sufficient accuracy, without need of more elaborate analysis, such as might be necessary in the Straits of Gibraltar, where the difference is only about $1\frac{1}{2}^{\circ}$.

Having carried out these observations at different depths in the Sea of Marmora and the Dardanelles, from below the Dardanelles Castles, a very interesting fact was ascertained,—namely, that nearly in proportion as the descending superficial current from the Black Sea diminished, so did the saline density of the water increase; and where there appeared to be no current (that is, below 40 fathoms in the Sea of Marmora, and below 20 fathoms in the Dardanelles) the density remained the same at all depths, and was that of the Mediterranean density.

Thus, in the Sea of Marmora, the density of the water in 40 fathoms, and from the bottom at 400 fathoms, was the same, viz. 29° by the hydrometer, and about corresponded with my observations on the Mediterranean density in general down to 2000 fathoms, except in one instance, Crete and Lybia, when it was 30° at that depth, and at another only $28\frac{3}{4}$ at the depth of 1200 fathoms, with $29\frac{1}{2}$ above*.

Then in the Dardanelles the current was found to cease at 20 fathoms,

* See Crete, vol. ii. p. 332.

and the maximum, or Mediterranean density of 29° , was found to be constant from that depth downwards, whilst the surface showed the same density as that of the Sea of Marmora, viz. 20° , the Bosphorus being about 14° , and Black Sea surface $13\frac{1}{2}^{\circ}$, and about 15° below 100 fathoms. Therefore in this part of the Dardanelles, between the surface and the depth of 20 fathoms, there was an increasing density from 20° – 29° as the result of the intermixture in the deeps of the narrows and from the encircling eddy or return-current by the south shore in that part, as invariably occurs; and in proportion as the density of the water increased to that depth, so did the rate of the current decrease, as shown in the following Table:—

Depths.	Density.	Temperature.	Current.	Remarks.
Surface	20	50	Rate of $2\frac{1}{4}$ knots.	At twenty miles westward of the Dardanelles, in the \mathcal{E} gean, the surface-density was the same as the Mediterranean.
5 fathoms.....	22	50	Rate of $1\frac{1}{4}$ knot.	
10 fathoms.....	25	53	Rate of $\frac{1}{2}$ knot.	
15 fathoms.....	27	55	Rate of $\frac{1}{4}$ knot.	
20 fathoms.....	28	55	Rate very slight.	
40 fathoms.....	29	54	No current.	

The plan I adopted for ascertaining whether any such undercurrents existed, as well as of the rate of the surface-current in descending depths, was as follows:—

A suspended sinker, or current-anchor, was made in several forms to test the most simple and effective form. Sometimes a vertical cross was used, which was formed of boards, so as to offer a resisting surface every way when hanging vertically with the weight or lead attached to its base. More generally a large weighted disk or enlarged log-ship was used, and when weighted with a weight consistent with size of the disk, and also of the line and float, was slung like a kite, the weight being in the place of the tail. When *thus slung* it would of course hang nearly vertical in all depths, and offer a sufficient resistance to prevent its being moved by the friction of the surface-current upon the float used to suspend it. It will be thus seen that the operation of testing slow currents in the deeps is one of great delicacy, and therefore requires great nicety and care in the mode and apparatus for doing it, for scientific dependence and aims.

The float that, after many experiments, I found to answer best was one made of thin copper or block-tin, in the form of an elongated air-tight cylinder, 4 or 5 feet in length, and pointed at *one* end to offer least resistance to the surface-current passing it. The other end was truncated or flat, where two loops were fixed in which a small rod or staff with a flag could be placed, to render its position conspicuous, without adding to its resistance or weight. The suspended kite, or current-anchor, was then weighted to about one-third of what the float would bear in perfectly still water, without being wholly immersed.

*Remarks and Experiments on the superficial and supposed under-currents of the Mediterranean, &c.**

It can be easily understood that, if a superficial current of 1 knot is observed to pass a float attached to a line which has a sinker or anchor at the bottom, and also if the same amount of surface-current of 1 knot passes a float which is attached to a suspended sinker or current-anchor suspended halfway or at any depth, the sinker thus held suspended by the surface-float is evidently as stationary as the one at the bottom, and therefore it must be in perfectly still water, whatever the depth may be ; consequently the superficial current does not descend to that depth.

Also, if another suspended sinker or current-anchor is lowered down a few fathoms (say 10 fathoms) from the surface, and the float attached to it has *no current* passing it, and consequently drifts away from the stationary float attached to the bottom and near which it was lowered, it is quite clear that the suspended sinker and its float are within the same influence, in fact in the same amount of current.

Again, if the suspended sinker be lowered to 20 fathoms by the side of the stationary float, and a current of about half a knot be then observed passing its float, although still drifting away from the stationary float, then, as this latter float showed a current of 1 knot passing it, and the float of the suspended sinker in 20 fathoms only showed a current of half a knot, it is also clear that the current-anchor or suspended sinker was in a current of only half the speed of the superficial current, viz. of half a knot only.

Also, if the suspended sinker be lowered to 50 fathoms, and the superficial current passing its float be three-fourths of that passing the float attached to the bottom, or running three-quarters of a knot, it is evident that the current at the depth of 50 fathoms was three-fourths less than the surface-current, or only running at the rate of one-fourth of a knot.

In this manner, then, my experiments were carried out at different depths, and at different times, in the Archipelago, Sea of Marmora, and Dardanelles, as being favourable positions for testing the superficial currents, and also of the existence of undercurrents, if any existed in these straits and seas, as some have supposed.

Thus, on the morning of December 19th, 1857, I hove to in H.M.S. 'Medina,' between Rodosto and Marmora Island, near the eastern entrance to the Dardanelles, and from a boat sounded with a shot and seine-twine in 350 fathoms ; I then attached to the twine a piece of light wood as a stationary float. The superficial current was then tested by the common log-reel, run out from a boat kept stationary abreast of the stationary float, when a current of 0·9 of a knot was observed to be running towards the Dardanelles.

Experiments for trying the rate of the current at different depths were then made in the following manner :—A flat piece of wood like a log-ship

* Travels and Researches in Crete, vol. ii. p. 333.

on a large scale was weighted with a piece of lead of about 4 pounds, and slung by its corners like a kite, so as to act as a suspended anchor or sinker, and was lowered to a depth of 5 fathoms ; and as no current was observed passing the float when the sinker was at this depth, it follows that both float and sinker were in the same amount of current, or in the upper stratum of the current ; that is, both were drifting along in a current of 0·9 of a mile per hour.

It was then lowered to 10 fathoms, when a sensible current was observable passing the buoy or float, which measured about 0·3 of a knot per hour, or just one-third of the rate of current running past the float attached to the shot at the bottom in 350 fathoms ; therefore the rate of current at 10 fathoms was ascertained to be only 0·6 of a knot per hour.

The suspended sinker was then lowered to 20 fathoms, when there appeared a much greater amount of current passing the float, and the rate was found by the log-line to be about 0·5 of a mile per hour, thus showing that the float of the suspended sinker was held in check by the sinker being in a current about half that of the surface-current, or running at only about 0·4 of a mile per hour at 20 fathoms' depth.

Again, on lowering the sinker to 30 fathoms there was immediately observed an increase of the superficial current passing its float, showing, therefore, a still diminishing current as the suspended sinker descended, since it was thus kept more stationary.

Then at 40, 50, 100, 200, and 300 fathoms the rate of the current passing the float of the suspended sinker was about 0·8 of a mile,—that is, nearly that of the surface-current when in all depths below 40 fathoms, so that an outward current of 0·1 of a knot per hour would appear to exist there ; but in reality this was the result of using in this instance a too bulky float, by which the suspended sinker was dragged along at that rate ; still water, therefore, undoubtedly existed below 40 fathoms, as confirmed by the density experiments and others in those depths.

This result was given to show the confusion and error almost sure to arise from using bulky apparatus as a float, that offered too much resistance to the surface-current ; and a double source of confusion and error is the sure result if the observation is also carried out with any wind and sea.

No undercurrent could therefore have existed here on the eastern approach to the Dardanelles as many have imagined ; for an undercurrent being an opposite current to the current observed running past the fixed float, the current then observed running past the float attached to the suspended sinker or current-anchor would have measured a greater rate than that passing the fixed float. Moreover, also, as the suspended sinker would have been dragged along by the undercurrent in an *opposite* direction to the surface-current, its float would have presented the singular phenomenon of going to windward of the fixed one ; or, in other words, *would have run up against the stream instead of down with the surface-stream*. This, on a slight consideration of the phenomena, will be evident, and the delicacy

of the operation too, especially when testing any very slow ocean-current, of only 0·1 of a mile per hour or less, which is quite practicable, as I have frequently done. And it is rendered sure and easy by always having a fixed float with sinker at the bottom as a point of reference, even where objects are near and charts are correct, and also by using very fine twine as a log-line to each float, and allowing it to run out from five to ten times the usual interval in measuring a ship's rate. The diagram on p. 537 will illustrate the matter*.

The diagram referred to will illustrate the plan, and the result will be more comprehensible and satisfactory, because sooner completed, if we suppose the trials to have been made from two or three boats (instead of from only one boat), each being provided with one or two buoys and suspended sinkers to suit, and with lines to each arranged for different depths; and if also *a fine log-line is attached to each float* for measuring the distance it drifts in a given time, and from it the rate of each float, the boats being always kept abreast of each other. For although each float, from the different times each suspended sinker will take to reach its intended depth, will have drifted away to some small varying distance from the boats and stationary float, these varying intervals will correspond to the stray line paid out in measuring a ship's rate through the water; and being noted in the usual way (as the rate-lines will be duly marked at intervals of 10 feet), or by a piece of cork or rag attached to each, when at the given signal the interval, by watch or glass, is simultaneously commenced to be noted, the deduction of this stray portion from the whole length run out in the arranged interval of five or ten minutes or more, according to the speed of the current, will give the surface-rate of each, and the consequent rates of the currents in which the suspended sinkers are lowered are easily deduced from them.

Now all these observations showed no undercurrent into the Black Sea, such as Dr. Carpenter maintains must necessarily exist to restore the saline density of that sea; if, therefore, I can show how that density is otherwise maintained, and by a more natural and more universal process and influence in connexion with ocean movements in all seas, the theory of universal undercurrents, as a great circulating medium for recovering the equilibrium, is deprived of its main support,—the main ground upon which it is advocated as a predicted necessity.

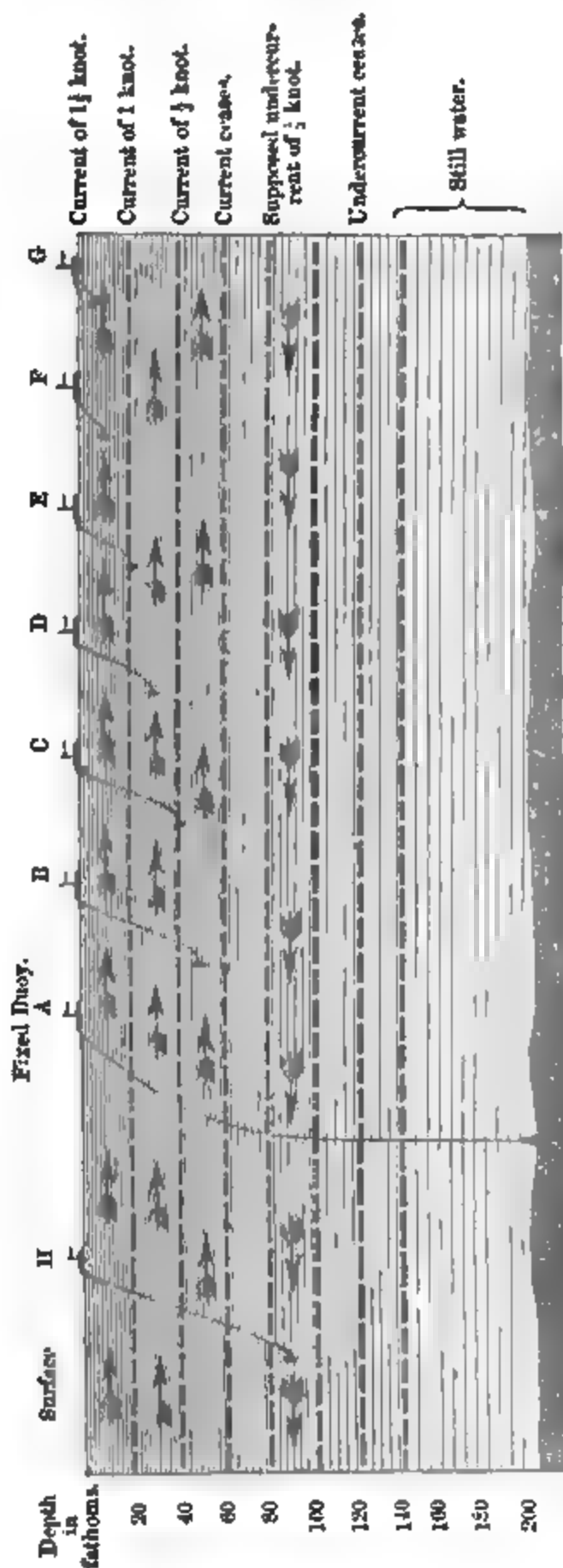
To better understand the remarks and facts that will follow upon the densities and currents of the Black Sea Straits, I must briefly notice the physical features influencing them.

First, the Black Sea attains a depth of about 1000 fathoms and more over a large portion of its area. The Sea of Marmora attains between 400 and 500 fathoms, and the Ægean about the same; whilst the Dardanelles and Bosphorus do not exceed 20 and 40 fathoms.

The facts, then, are simply that, although a diluted current of Black Sea

* Crete, vol. ii. Appendix, p. 338.

Diagram showing the mode of testing the existence, direction, and rate of surface- and undercurrents in the deeps of the sea.



B, C, D, E, F, G, and H, relative positions of the floats to each suspended sinker, after being simultaneously dropped near the fixed float, A, and allowed to drift in their respective currents for five minutes.

Thus, if a surface-current of $1\frac{1}{2}$ knot was found to be passing the fixed float A, and the floats B, C, D, E, F, G had reached those positions at the end of five minutes' interval, their suspended sinkers or drags were in diminishing rates of the surface-current; but the float H, from being dragged in the opposite direction, had evidently reached an undercurrent with its suspended sinker or current-drag.

water flows for a great part of the year as a skimming surface movement around and across the Sea of Marmora and through the Dardanelles, and at no greater depth than from 20 to 40 fathoms, viz. that of the barrier ridges between the Black Sea and Ægean, there occurs for several days in the year a strong *reverse current* into the Black Sea from the Ægean. This reverse current is frequent during the autumn and winter months, when the Black Sea rivers are at their lowest, so that the Black Sea level is then frequently overbalanced by the pressure of westerly gales in the Mediterranean or Ægean.

Therefore it is this recurring *return current* into the Black Sea that maintains, as I found to be the curious or interesting fact, the Ægean or Mediterranean density in the deeps of the Sea of Marmora in all depths below about 40 fathoms, and therefore that restores also the lost salinity of the Black Sea through the flow of diluted water so prevalent as a surface-current from it.

This return-current occurs sometimes for two or three days at a time, and occasionally at a rate even greater than the general outflowing current of 2 and 3 knots. The same occurs at the Kertch Straits.

Therefore, instead of the Black Sea being washed fresh, unless there was an undercurrent to restore it, as Dr. Carpenter argues, its normal density is restored by the surface return-current, as also that of the Sea of Azof.

But I am induced to believe, as I have elsewhere stated*, that the Black Sea has not become a diluted or brackish sea from a previously salt sea, but, on the contrary, from a freshwater lake has become a brackish one.

This I infer to be the fact from the latest deposits existing around the shores of the Black Sea, Sea of Marmora, and Dardanelles being of freshwater origin, a large *Dreissena* and a freshwater Cockle in them being mistaken previously for a Mussel and *Cardium* until I discovered the error.

Having thus shown these physical conditions *as facts* in connexion with the Black Sea, and that the undercurrent theory is a fallacy where it was expected and insisted upon as being a predicted necessity, as a constant counterbalance to the surface-outflow, from the great difference in the densities between the Black Sea and Ægean, viz. 13° and 29°, as shown by a common hydrometer, I shall now refer to the Baltic Sea and German Ocean, where even a greater difference in the saline density exists; and I shall be able to show also that precisely the same conditions of outflow and inflow exist there, and that it is surface-currents only which restore the lost salinity, and that no undercurrent is necessary there to prevent the saltiness of the Baltic from being washed out; that, in fact, no undercurrent system does exist as a means of restoring the equilibrium and maintaining the normal conditions of that sea. This I shall show from Dr. Forchhammer's observations; but he has evidently mistaken the right

* Crete, vol. ii. p. 349.

conclusions to be drawn from the facts he gives regarding the densities and currents, and thus led Dr. Carpenter to adopt them as follows:—

“Now if it can be shown that a similar *vertical circulation* is maintained in the opposite direction, when the conditions of the case are altogether reversed, the explanation above given may, it is submitted, be regarded as having a valid title to acceptance. Such a converse case is presented by the Baltic, an inland basin which communicates with the German Ocean by three channels—the Sound, the Great Belt, and the Little Belt, of which the Sound is the principal. The amount of fresh water discharged into the Baltic is largely in excess of the quantity lost from its surface by evaporation; and thus its *level* would be continually *raised* if it were not kept down by a constant surface-current, which passes outwards through the channels just mentioned. But the influx of fresh water *reduces* the *density* of the Baltic water; and as the water which the outward current is continually carrying off contains a large quantity of salt, there would be a progressive reduction of that density, so that the basin would at last come to be filled with *fresh* water if it were not for a deeper inflow. Such an inflow of denser water might be predicted on Principle VI. as a physical necessity, arising from the constant want of equilibrium between the *lighter* column at the Baltic end of the Sound and the *heavier* column at its outlet in the German Ocean; and that such an undercurrent *into* the Baltic has an actual existence, was proved two hundred years ago by an experiment of the same kind as that by which we have recently proved the existence of an undercurrent *out of* the Mediterranean. This experiment is cited by Dr. Smith (*loc. cit*) in his discussion of the Gibraltar current, as supplying an analogical argument for his hypothesis of the existence of an undercurrent in the Strait of Gibraltar; but he does not make any attempt to assign a physical cause for the movement in either case”*.—*Proceedings of Royal Society*, vol. xix. p. 213.

I need only add, as a remark to this latter part, that a greater density below was no proof of an undercurrent, as shown by the contrary fact in the Dardanelles and Sea of Marmora.

In his paper in the Philosophical Transactions for 1865, “On the Composition of Sea-water in the different parts of the Ocean,” in page 230 Dr. Forchhammer says, “In the Baltic likewise the water from the deeps contains more salt than that from the surface. The upper-current goes generally (not always) out of the Baltic, the reverse of the Mediterranean. The cause is evident, the excess of atmospheric water in the Baltic from the rivers surrounding it..... With the assistance of Captain Prosilius, in 1846, who commanded the vessel stationed at Elsinore, the surface-current was observed on 134 days, from 27th April to 11th September; of which on 24 days it ran from the north, on 86 from the south, and on 24 days there was no surface-current at all.”

* “Prof. Forchhammer fully confirms Dr. Smith's statement, and further shows that the water which thus returns to the Baltic has the density of the Sound water, the surface-current being formed of the much lighter Baltic water.”

“The mean quantity of salt for the current from the north was 15·994 per 1000; that for the current from the south 11·801; that for the period when there was no current at all 13·342. Once a week a sample was taken from the bottom. The mean of nineteen observations was 19·002, which is rather under than above the real mean, and proves that it is water from the Kattegat which runs at the bottom of the Sound.”

“Experiments once a week, at Copenhagen, from 3rd March to 25th April, 1852, gave as follows:—for the surface 15·845 per 1000 salt, for the bottom of the harbour 17·546 ditto, which seemed to prove that the undercurrent at that season reached Copenhagen.”

In page 222, Dr. Forchhammer also shows the salinity of the German Ocean, the Kattegat, and Sound, and in the eastern and western parts of the Baltic as follows:—

“German Ocean, mean of six analyses, 32·823. In the Kattegat and Sound the quantity of salt is very variable; a northerly wind makes it richer in salt than with a southerly wind. The mean of six analyses and 141 observations, in which only chlorine was determined, gives 16·230 per 1000, the maximum 23·243, and the minimum 10·869. In the Baltic the salinity varies very much, and is of course less in the eastern than the western portions. I found the mean 4·931 per 1000 salt, the maximum 7·481 between Bornholm and Sweden, the minimum at Kronstadt 0·610 per 1000 salt.”

The relative saline densities are therefore as follows:—

German Ocean	32·823 mean.
Kattegat and Sound	16·230 „
Baltic	4·931 „
Baltic, between Bornholm and Sweden	7·481 maximum.

Dr. Forchhammer's observations are all upon the densities at different depths in the Sound, and some few temperatures at the surface and bottom. There were, unfortunately, none upon the rate of the surface-current in descending depths, as carried out by me in the Mediterranean and Black Sea Straits, or without doubt he would have found the same results, that is, by the fact recorded by him of the density being greater at the bottom of the Sound than at the surface, he would have ascertained a corresponding diminution in the outward rate of current in descent from the surface. But he has inferred, from the fact of there being a slight increase of density at the bottom of Copenhagen harbour, viz. of 17·546 at the bottom and of only 15·845 at the surface, that there was necessarily an undercurrent there from the denser water of the Kattegat and German Ocean; for he says that at Copenhagen the density was “For the surface 15·845 per 1000 salt, for bottom of harbour 17·546 per 1000, which seemed to prove that the undercurrent at that season reached Copenhagen.” Thus quite overlooking the remarks he has made upon fluctuations in the saline densities produced at times, both in the Baltic and Kattegat, from the influence of winds, and forgetting, too, “that for twenty-four days out of the

134 the surface-current was running from the north at Elsinore," that is, into the Baltic from the Kattegat and German Ocean, and of course then doing what I have shown to occur in the Sea of Marmora and Black Sea, with every recurring and return-current into them, viz. restoring the lost saline density of the surface and deeps of those seas. Now the proportion of the inward return surface-current from the denser seas is very much greater in the Baltic Straits than in the Black Sea Straits, and, moreover, the depth on the dividing ridges of the Sound and Great Belt leading into the Baltic does not exceed 9 fathoms, or about 50 feet only; and thus, even here (notwithstanding the great supply of fresh water from the Baltic rivers into the Baltic between April and September, when the observations were made, and, therefore, the time of greatest supply in the whole year), the outside influences of wind &c. so outbalanced the surface-level between the Baltic and German Ocean, as to produce twenty-four days inward current in that period, and twenty-four days without any current, then stopping the Baltic outflow in fact. So that Dr. Forchhammer himself shows that the phenomena of the currents to and from the Baltic are, at one time, a mass of diluted water outwards over the very shallow barrier forming the straits, and then a run of the Kattegat denser water inwards for the restoration of the normal saltness. What need, then, was there for looking to an undercurrent as the great source of such a resupply, when it was evidently produced by a greater agency during the twenty-four days return surface-current inwards, when the German Ocean must have stood at a higher level than the Baltic? One more remark is necessary to show another fallacious conclusion of Dr. Forchhammer in favour of the undercurrent theory. In p. 233 he says, "I observed on the 2nd of March, 1850, the temperature of the undercurrent at a depth of 108 feet to be $36^{\circ}\cdot8$ Fahr., while the temperature of the surface was $34^{\circ}\cdot9$ Fahr.,"—thus intimating that because he found a higher temperature and density in 108 feet at Elsinore it positively indicated an undercurrent from the German Ocean or Kattegat, the conditions of density and temperature being those of the outside sea and not of the Baltic. But these were the natural conditions of things there, because in the depth of 108 feet at Elsinore he was in a depth more than twice that of the submarine shallow barrier that separates the Baltic from the Kattegat, and on the Kattegat side of the ridge. The barrier or ridge is at the southern end of both the Great Belt and Sound, and is thirty miles to the south of Elsinore, which is therefore on the German Ocean side of the ridge; and, moreover, the depth of 108 feet at Elsinore was in the greatest depth of the Kattegat anywhere within fifty or sixty miles northward, and the barrier ridge is only 30 feet deep here. The undercurrent view, then, has no ground of support from this fact; but the contrary, for the increase of temperature and density was found exactly where it was natural to expect it, that is, in the region of depths below the comparatively shallow barrier of about 50 feet in depth which separates the Baltic from the Kattegat; and especially so as

it was in the depth of 108 feet, so considerably below the skimming surface-current of lighter water flowing from the Baltic. The denser and warmer water thus found in 108 feet was therefore a continuity of the warmer and denser waters of the deeps of the Kattegat and German Ocean. But the Kattegat being shallow, with a wide entrance between it and the German Ocean, it has no deep trough so much below the separating barrier for retaining still water as in the Sea of Marmora ; its waters would, therefore, fluctuate in density throughout, between the densities of the Baltic and German Ocean.

As Dr. Carpenter has made the fallacy regarding the Baltic conditions on the authority of Prof. Forchhammer's statements and opinions the strongest reason and basis of his enlarged theory of universal undercurrents, I must refer to another circumstance, another error of the late Professor in assuming the existence of an undercurrent at the Sound flowing into the Baltic when the surface-current was running out, viz. the fact often observed in the Sound, that ships of deep draught are frequently carried past the lighter draught ship when sailing together into the Baltic through the Sound*.

There is, however, another and more probable explanation of the phenomenon, namely, that the deep-draught ships are, by the lowness of their keels in comparison with that of the light-draught craft, under a lesser influence of the outward surface-current, through the whole strength of the current not descending to the depth of their greater draught, as surface-currents always diminish in descent. The mean strength of a current felt by a vessel drawing twenty feet of water would consequently be less than the mean strength felt by one drawing only eight or ten feet ; this will be evident, especially as the surface-current from the Baltic cannot descend much below the depth of sixty feet, viz. that of the Barrier ridge across the Sound and Great and Little Belts, moreover there is no tidal influence there to force or confuse the outflowing surface-current from the Baltic.

Judging from these explanations and facts there appears to be really no evidence, from the observations of Prof. Forchhammer, that an undercurrent is a real necessity for the restoration of the lost salinity of the Baltic any more than in the Black Sea ; but, on the contrary, that the evidences and facts are confirmatory of there being no such undercurrent, and no such necessity for one in either case.

Therefore, finding the undercurrent theory fallacious in both instances, I have no faith in its application to the Ocean as a grand law of interchange between surface and deeps, pole and equator, as the great universal movement advocated by Dr. Carpenter. I am therefore induced here, from the apparent importance of some views and facts bearing on the question, to reiterate the following arguments in support of this opinion, which were given elsewhere †, commencing the discussion with the interesting facts regarding the high normal temperature of the deeps of the

* Phil. Trans. 1865, p. 231.

† Crete, vol. ii. Appendix.

Mediterranean as compared with the deeps of the Atlantic Ocean on the west side of the 150- or 160-fathom barrier that separates the one from the other at the western embouchure of the Straits of Gibraltar.

The very high temperature of the depths of the Mediterranean below about 200 fathoms, in all seasons, as compared with that of the Atlantic and Pacific (where, according to Ross, Belcher, Denham, Pullen, and others, it seems to remain at about $39\frac{1}{2}^{\circ}$ Fahr. * in all latitudes between the Arctic and Antarctic zones) results apparently from its insulation from the Atlantic deeps by the 150-fathom bank or submarine ridge across the entrance of the Gibraltar Straits, and thus appears to have settled into a mean resulting from a small terrestrial influence from below and the large solar influence above, since the normal temperature is constantly at 59° † at all depths below 100 to 200 fathoms.

The fluctuations of temperature in the Mediterranean Sea are consequently confined to this upper zone of about 100 fathoms, in which the temperature varies with the seasons, being in the summer and autumn from 10° to 20° higher than the normal temperature, whilst in winter it rises up at the surface to the normal temperature of $59^{\circ}-4^{\circ}$, viz. 55° ; and is then even sometimes 10° lower at the surface and a few fathoms below it, viz. in January and February, the coldest months.

In the same parallel in the Atlantic the normal temperature of $39\frac{1}{2}^{\circ}-4^{\circ}$ is not reached in summer in less than 1000, or in 1200 fathoms in the tropics. This is a peculiar condition of the two seas deserving notice. Had the normal temperature of the Mediterranean been as low as that of the Atlantic, the superficial influence would no doubt have extended downwards to the same depths as in the Atlantic. Upon the first consideration of these facts, however, the inference seems to be, that the Atlantic deeps are under the influence of cooling-down undercurrents from the poles. But appreciable undercurrent movements as a universal movement (such as the theory advocates) I have no belief in, except, probably, where two great streams meet, such as the Arctic current and the Gulf-stream.

It has been well shown, too, in support of this opinion, during the soundings taken across the Atlantic, that perfectly still water reigns in a large area of its deeps, by the fact of the sounding-line, on several occasions, having coiled itself upon the sinker when some 200 or 300 fathoms more than the actual depth had been accidentally or intentionally paid out from the ship, and thus the coils came up in a bunch together *round the deep-sea lead*, around which the line had become coiled as it stood upright in the soft ooze or clay usual in great depths. This result was, therefore, a most excellent test for showing that no appreciable movement or current existed in a very considerable portion of those depths; for it proved that the line must have descended in the lower depths quite vertically when slack, with

* -3° or 4° as the constant error now to be applied to all the earlier deep-sea temperatures.

† —the 3° or 4° as the constant error.

the lead at the bottom, as well as before it reached the bottom, so that no incline of the line from a perfectly vertical course of descent could have occurred for several hundred fathoms above the bottom; all must have been perfectly still water there, for the deviation of the line for a few inches only out of the perpendicular in the lower depths would have prevented the line from coiling itself around the upright lead, and so from this perfect stillness the lead returned to the surface with the excess of 200 fathoms coiled round it*. Now it is perfectly impossible that the ship could have been kept stationary over the same spot in the Atlantic, under the most favourable circumstances, even for a few minutes, much less so during the time a sounding-line takes to descend in about 2000 fathoms; for the combined influence of swell and of the smallest superficial current during this time would drift her from it considerably, as must be evident to every one. Hence it will be perceived that, unless perfectly still water existed in the lower depths, no coiling together of the excess of line paid out around the lead could occur; and as it occurred on several occasions, I have been led to interpret the fact, as did Capt. Dayman, as being a most interesting and satisfactory test, where it occurred, of the perfect stillness of the ocean deeps there.

It is shown by the few soundings that have been taken in the Atlantic, that probably a continuous depth of at least 2000 fathoms extends along it between the Arctic and Antarctic circles.

The consideration of the above points, then, opens up the question of how this low normal temperature of the Atlantic and Pacific deeps is retained in continuity, with a higher terrestrial temperature below, as generally supposed, and a higher atmospheric temperature above—whether it is chiefly, if not entirely, due to the *horizontal conduction of this low temperature* from the Arctic and Antarctic zones and seas during the long ages the present poles have been the sources of cold, combined also with the great density resulting from this low normal state, and consequent tendency of such cold and dense water to remain in the deeps (a view I am more inclined to accept), or whether entirely due to a continuing undercurrent movement between the poles and the equator, as others suppose.

I only touch upon the question here, and thus merely state, in reference to the undercurrent theory, that there seems to be an opposing difficulty in the first thought upon it,—first, because I conceive that the horizontal conduction of extreme cold can evidently occur in a continuing column of equable depth, such as exists in the ocean deeps, and when completely effected remain so, without requiring an appreciable undercurrent to maintain it; secondly, because the existence of such a current seems to require one of two conditions—either a much less density of the substratum of fluid in continuity before it, so as to cause a horizontal flow, or a pres-

* See Capt. Dayman's Report of deep-sea soundings across the Atlantic, pp. 7 & 8.

sure in that direction, from the greater density of the substratum at the source of its origin ; but the temperature of the greater depths that are in continuity seems to be of the same low normal condition below about 1000 or 1200 fathoms, so that there is no such difference to set up an appreciable horizontal movement in those deeps.

Although undercurrents undoubtedly exist in the atmosphere, and thus may lead to the possibility or belief in such general movements as a law of the deeps of the sea also, yet the modes in which the solar influence operates upon the two media are diametrically opposite. In the sea the rarefying influence of the sun commences from, and therefore remains at or near the surface, whilst in the atmosphere it commences from below, and therefore disturbs and causes the lower strata to ascend.

The sea is also a comparatively non-elastic fluid, whilst the air is the most elastic, and thus yields to every local influence, whether of heat or cold.

The isothermal temperature of the ocean deeps (viz. about $39\frac{1}{2}^{\circ}$ Fahr.) has been supposed to be that at which the water attains its greatest density, probably because it is found at the lowest tried depths of the Atlantic and Antarctic seas, and because of its being the temperature of greatest density of fresh water ; and therefore it has been said that a lower temperature made sea-water lighter, causing it to float upon that at the above-mentioned temperature.

But this is contradicted by the temperatures found by Sir E. Parry, and by the recent experiments of M. Edland, M. Despretz, and others, which seem to show that the greatest density of sea-water is attained between 22° and 25° Fahr.

It seems to me therefore (and I was impressed with the opinion before knowing of this fact and the statements that confirm it) that this isothermal temperature of $39\frac{1}{2}^{\circ}$ — 4° , found throughout the Antarctic deeps, is the settled mean temperature produced by the atmospheric influence upon these areas, as about 59° Fahr. is of the eastern basin of the Mediterranean, and about $55\frac{1}{2}^{\circ}$ Fahr. is of the deeps of the Greek Archipelago, and 54° for the Sea of Marmora*—this difference in similar depths occurring in consequence of the separation of the deeps of the two basins by a submerged but comparatively shallow ridge between them, as the Mediterranean deeps are separated from the Atlantic by the shallowest part of the Straits of Gibraltar, and with an isothermal temperature of 59° for the deeps on one side, and of $39\frac{1}{2}^{\circ}$ on the other, subject to the deduction of 4° or 5° from each.

These facts suggest the view that there really may not be an exact correspondence between the lowest temperatures of the Atlantic, Pacific, and Indian oceans, although, when a temperature in excess of or under $39\frac{1}{2}^{\circ}$ has been found, there has generally been supposed, since Sir James Ross's establishment of this as the normal temperature of the ocean deeps to be an error of observation, or a defect in the instrument used.

* -4° for each.

The foregoing quotations, and recapitulations of arguments and reasons from 'Researches in Crete,' which, in my humble judgment and experience, seemed to be sound, in opposition to the undercurrent theory as a grand circulation, and general and appreciable as a fact, I again offer in concluding these remarks, but with all due deference and diffidence, although I am strongly of opinion still, from my practical experience and investigations regarding surface- and deep-water currents, that differences of density and of level are more generally rectified by superficial and littoral movements, than by undercurrents running up hill or burrowing in mid-deeps. But if recognizable or measurable as a physical fact anywhere, it is only local and not universal, and is merely an atomic interchange of insensible amount in general, in the greater depths of the ocean or of inland seas.

On the Gibraltar Undercurrent.

There are also strong reasons for inducing me to dissent from Dr. Carpenter's conclusions as proofs of the undercurrent he asserts to having found indisputable proofs of in the Gibraltar Straits; for to my mind, on carefully considering the observations, as well as the means employed, and circumstances at the one trial (Station 64) which was accepted as an undoubted evidence of such an undercurrent, against the four others that showed no such result, there does not appear to be just grounds for asserting that it really exists, as a positive result of the trials; for in such a question of science the fact should be free of any ground of doubt.

In such a Strait as that of Gibraltar, however, where there are tidal influences combined with the general inset from the Atlantic, an undercurrent at certain times is a possibility; but, with all due deference to Dr. Carpenter, I cannot agree with him in inferring from the single and, to my experience, unsatisfactory result obtained at Station 64, "that a strong presumption may be fairly raised for the *constant* existence of such a return-current, though its force and amount are liable to variation," when the results of his four other trials, viz. two at and near Station 39, and one at 65 & 66, showed no undercurrent, the former being in the narrowest part of the Strait, and the latter over the shallow ridge that unites Europe with Africa, the average depth of which does not exceed apparently more than about 130 or 140 fathoms, although there are depths of 160 near to the African side. It extends across, between Cape Trafalgar and Cape Spartel, the two western capes of the Strait.

The width of the Strait between these two capes is $22\frac{1}{2}$ miles, and the width in its narrowest part near Tarifa is only $7\frac{1}{2}$ miles.

There is therefore a great convergence of the confining coasts and descending slopes to this part, and a necessarily convergence of the Atlantic tidal wave, as well as general inset of the Atlantic current.

In this constricted part of the strait also the greater portion of the depth (fully 5 miles of the $7\frac{1}{2}$ across) is more than twice as great as on

the barrier-ridge to the westward that separates the deeps of the Atlantic from the proper Mediterranean deeps.

Therefore, as there is here a great convergence or concentration of the Atlantic inset, here there would naturally be a deeper tendency of the inflowing current, as well as of an uprising of the lower part of it, where this concentration produced a more rapid commingling of converging waters, and a sort of boiling-up of parts of the deeper waters would be the natural result of this convergence and constriction. Colder waters would therefore come towards the surface, and *vice versa*.

Now Dr. Carpenter shows this to be the result here, although he does not recognize what appears to me to be the natural and simple explanation of the phenomenon as above given. He says in regard to this :—"It was not a little perplexing to find, when we had fairly entered the Strait and were proceeding along the mid-channel towards Gibraltar, that the surface-temperature of the sea fell still further to $66^{\circ}\cdot4$, whilst the temperature of the air rose to $76^{\circ}\cdot6$, thus showing the then unprecedented difference of $10^{\circ}\cdot2$ between the two ;" and on his return to the same part about two months afterwards, viz. at Station 64, he found the surface-temperature there 66° .

Now, if this be the true and simple explanation of the low surface-temperature over the position of greatest intermixture or boiling-up of the currents there, as I suggest and believe, we should expect the same thing to occur in some parts over the ridge which extends across the western entrance to the Straits, where the Atlantic current or inflow is also somewhat concentrated, or *first meets it as an obstruction*, and thus causes an uprise of the cold water from below to the surface. And, curious enough, the two surface-temperatures taken by Dr. Carpenter at this part, viz. at Stations 65 and 66, show in proof a much lower temperature, only 63° at the first of these, and 69° at the other. The temperature of 66° at Station 64 is therefore clearly a commingling of the uprisen cold waters over the barrier-ridge ; for the mean surface-temperature at 50 miles' distance, on the west side of the ridge, was $72\frac{1}{2}$ degrees, and from about 50 miles' distance from Gibraltar on the east of the Strait it was $73\frac{1}{2}$ degrees, that is, the mean of seven observations taken about the former distance by Mr. Gwyn Jeffreys, and of seven about the latter by Dr. Carpenter. Deductions from temperature and density in such positions as the narrows and over the barrier-ridges are therefore, to my mind, not reliable ; I experienced the same at the narrows of the Dardanelles, near the two Castles, and so carried out my observations in more normal conditions or tranquil areas, and in parts free of local disturbing influences that might tend also to divert the direction of the lower currents, as well as of those near the surface, and lead to erroneous conclusions favouring a bias for any theory or prediction.

As Station 65 was one of Dr. Carpenter's positions for trying for the under-current he asserts to exist, and as he has drawn some inferences in favour of

the positiveness of such an undercurrent there from the temperature and small difference in the densities, although the results did not show it by the current-drag operation, I am under the necessity of referring to it. He says, paragraph 67, page 182, "We commenced our observations on the morning of October 1st at the point of greatest depth (Station 65). The temperature of the surface at 6 A.M. was only 63° , which was at least 8° lower than the average temperature at that hour within the Mediterranean. The bottom-temperature at 198 fathoms was $54^{\circ}\cdot5$, and the specific gravity of the bottom-water was 1028.2. The coincidence both in temperature and specific gravity with the bottom-water at Station 64 was thus very close. The place of the ship having been determined by angles taken with the shore, the rate of the surface-movement was tested as on former occasions, and was found to be 1.277 mile per hour, its direction being E. $\frac{1}{2}$ S. The 'current-drag' was then sunk to 150 fathoms, the greatest depth at which it was thought safe to use it; and the boat from which it was suspended moved E. $\frac{3}{4}$ N. at the rate of 0.840 mile per hour. This observation indicated a very considerable retardation of the rate of inflow, but gave no evidence of an outflow. It did not, however, negative the inference deducible from the temperature, and still more from the specific gravity, of the water beneath, that an outflow takes place in that lowest stratum which we could not test by the 'current-drag.'"

The remark I feel it necessary to make is, that although the "current-drag" showed no undercurrent here in 150 fathoms in a depth of 198 fathoms, but, on the contrary, there appeared to be an E. $\frac{3}{4}$ N. current at that depth of 0.84, or about $\frac{3}{4}$ mile per hour, yet against this result Dr. Carpenter insists that "it did not, however, negative the inference deducible from the temperature, and still more from the specific gravity, of the water beneath, that an outflow takes place in the lowest stratum." Now, according to the depth, the sounding-drag when down in 150 fathoms was nearly down to the level of the barrier-ridge extending across the Straits there; and moreover, from the Station being where the depth was so great as 198 fathoms, it was on the *west side of the barrier* and considerably below it. The temperature in that depth also, being $54^{\circ}\cdot5$, corresponded closely with a temperature obtained by Mr. Jeffreys at Station 37, a little more to the westward, in 190 fathoms, which was there $53^{\circ}\cdot7$, whilst on the Mediterranean side, in 181 fathoms at Station 63, the temperature was $54^{\circ}\cdot7$; so that there was nothing abnormal in these temperatures at about the same depths, on different sides of the barrier-ridge, viz. that of a degree only in one, and in the other on the Atlantic side of about $\frac{3}{4}$ of a degree lower temperature than that of the Mediterranean side, where it was at its normal temperature of the deeps on that side; for on the Atlantic side of the barrier the temperature lowers gradually down to its normal depth of about $39\frac{1}{2}^{\circ}$ in the deeper regions, being at Station 35 in 335 fathoms $51^{\circ}\cdot5$ at about 30 or 40 miles to the westward of the one at 65, where the "current-drag" operation for testing the current was taken,

but its result ignored by Dr. Carpenter in favour of the supposed abnormal conditions of temperature and density there. Therefore I fail in being able to agree with Dr. Carpenter's predilection for the density and temperature there, against the "current-drag" indications. If therefore he ignores the "current-drag" test here, he must still more ignore the result at Station 64 by the same means, by boat and basket, and under still more unfavourable circumstances for doing it, when he so sanguinely relies upon it as "a *conclusive proof* that there was at this time a return-current in the mid-channel of this narrowest part of the Strait, from the Mediterranean towards the Atlantic, flowing beneath the constant surface-stream from the Atlantic into the Mediterranean."

For, with the boat and basket as a means for testing the surface and undercurrent, and without a fixed float attached to the bottom as a stationary point of reference for measuring the rates from, instead of by angles upon a chart of small scale, I cannot, from my experience of such operations (and I do not know any one who has had more experience or given more consideration to the subject for ascertaining the proper or best mode of doing it), agree that the result at Station 64 was a satisfactory or sufficient proof of such an undercurrent outflow as Dr. Carpenter contends for. For with a ship's cutter as a float, and with a force of wind of 4 against the surface-current, and producing so much sea (for Dr. Carpenter states it was necessary in consequence to use a larger boat than before, and not leave it to drift without a crew as on former trials), with these two forces, of wind and short sea together, acting in the presumed undercurrent direction, the result certainly cannot be accepted as a "conclusive proof" of such an undercurrent in 250 fathoms of 0·400, or nearly half a mile per hour. The conclusion to my mind was that the bulky boat, from being exposed most probably nearly broadside on to an easterly wind, and therefore following swell, was drifting faster than the inflowing surface-current from the westward, and thus drew the "current-drag" some little distance to the westward, against the $1\frac{3}{4}$ -knot surface-current, the "current-drag" being probably in still water in 250 fathoms. I am sure that in this view I shall have many scientific men, landmen as well as nautical, in full agreement with me, and that for the solution of a question of physical science, and in support of such large views as advocated by Dr. Carpenter, the result was not conclusive.

One more remark touching the "undercurrent flow up-hill" theory of Dr. Carpenter as the result of the 1028·1 specific gravity and of 55°·3 at the bottom at Station 67, in 188 fathoms. Now both these results are apparently to me not abnormal conditions, as compared with the other results in about the same depths, as I have shown in commenting upon the opinions following the density and temperature at Station 65; for I can only conclude from Dr. Carpenter's remarks that the position given of Station 67 in 188 fathoms was evidently on the *west side* of the barrier-ridge, the down-hill or Atlantic side of it, as the line of his section on the Chart of the

Strait shows, and not on the east or *up-hill* side, for an undercurrent coming from the Mediterranean, even if such existed there as an undercurrent. But I must give my reason for not considering this temperature and density at Station 67 as abnormal; for the position being clearly several miles on the Atlantic side of the ridge*, we should expect to find proximate Atlantic conditions on that side. Now, as Dr. Carpenter has no densities between Lisbon and the Straits, except at Station 67, he has no true comparison with the Atlantic conditions of either the surface or the deep water at that part; for the lighter density of the surface-water of the Straits is apparently due to its being a diluted or lowered condition of that of the Atlantic in the same parallel from the influence of the two large Spanish rivers, the Guadiana and Guadalquivir, which fall into the sea so near the entrance to the Straits: not so, however, the density in the depths of 188 fathoms; for there we should expect to find the normal density nearly of the proximate part of the Atlantic, which, if denser than the Atlantic in general, the same would be found in the deeper waters drawn from it by the indraft current into the Mediterranean, the river influence being confined to the surface and being also drawn into the Straits.

Then in regard to the specific gravity of 1028.1 at the bottom, which induced Dr. Carpenter to consider it to be Mediterranean water and not Atlantic, because some slight degree heavier than the mean of the Atlantic found by him between Lisbon and England, I am induced to believe, from Dr. Forchhammer's researches, that such a density is about the normal condition of the Atlantic deeps near the African coast in this parallel; for he shows that the maximum salinity of the Atlantic lies to the south-west of the Straits, about the parallel of 24° and up to about 36° north latitude, and some 300 miles only distant from the African coast, where he says it is 37.908 per 1000, and that this salinity is nowhere exceeded in the Mediterranean, but where its abnormal maximum density between Crete and the Libyan coast is found, which he shows is only exceeded in the whole ocean by the density found in the Red Sea†. This great salinity off the Morocco coast he attributes to the absence of rivers upon it; therefore it seems to me that as we have a source for a salinity as great as that of the mean salinity of the Mediterranean so near, on the outside of the Straits, we have no proof that the water of 1028.1 specific gravity, found by Dr. Carpenter at the depth of 188 fathoms at Station 67, and clearly on the outside of the barrier-ridge, is not Atlantic water, instead of being Mediterranean water, as he concluded, and concluded from it also that there was an "up-hill outflow" as a necessary result.

This, however, is an excusable oversight or misunderstanding of the conditions, as, with all due deference, it seems to me to be, in one not familiar with the indications of a few scattered soundings on a chart of the probable line of direction of the crest of a submerged ridge.

* See Chart of the Cruise of H.M.S. 'Porcupine.'

† See Phil. Trans. 1865, p. 220.

Postscript, June 23rd, 1871.

As the undercurrent theory, in its larger view, as first put forth by Capt. Maury, will remain a source of error still for the misguidance of the physical geographer and philosopher, whilst the fallacy or mistaken facts also remain uncontradicted, upon which it was mainly and originally founded by the eminent author of the 'Physical Geography of the Sea,' it is therefore now necessary for me to show, after what I have previously written on the question, that the assertion of an undercurrent of from 1 to $1\frac{1}{2}$ knot per hour in the Atlantic as counter to a surface-current of much smaller amount on the outside of the Gulf-stream, is based upon a mistaken estimate of the results of the experiments that were supposed to indicate such an undercurrent.

Capt. Maury says, in p. 141, 'Physical Geography of the Sea,' when discussing his undercurrent views in the chapter headed "Undercurrents :"—

"Lieut. J. C. Walsh, of the United States schooner 'Taney,' and Lieut. S. P. Lee, in the United States brig 'Dolphin,' both, while they were carrying on a system of observations in connexion with the wind and current charts, had their attention directed to the subject of submarine currents. They made some interesting experiments on the subject. A block of wood was loaded to sinking, and by means of a fishing-line or a bit of twine let down to the depth of 100 or 500 fathoms; a small float, just sufficient to keep the block from sinking further, was then tied to the line, and the whole let go from the boat.

"To use their own expression, it was wonderful, indeed, to see this *barrega* move off against wind and sea and surface-current at the rate of over one knot an hour as was generally the case, and on one occasion as much as $1\frac{1}{2}$ knot. The men in the boat could not repress exclamations of surprise; for it really appeared as if some monster of the deep had hold of the weight below, and was walking off with the line. Both officers and men were amazed at the sight."

In paragraph 273 he says, "It may, therefore, without doing violence to the rules of philosophical investigation, be conjectured that the equilibrium of all the seas is preserved, to a greater or less extent, by this system of currents and counter currents at and below the surface. If we except the tides and the partial currents of the sea, such as those that may be created by the wind, we may lay it down as a rule that all the currents of the ocean owe their origin to difference of specific gravity between sea-water at one place and sea-water at another; for whenever there is such a difference, whether it be owing to difference of temperature alone or difference of saltness, &c., it is a difference that disturbs equilibrium, and currents are the consequence. The heavier water gives towards the lighter, and the lighter whence the heavier comes; for two fluids differing in specific gravity, and standing at the same level, cannot balance each other."

From the above reasonings, it is clear that the eminent author, from the supposition that a great undercurrent movement in the Atlantic had been discovered as the result of the observations and experiments of Lieutenants Walsh and Lee, was induced to propound his fascinating but fallacious theory regarding the origin of "all the currents of the ocean" being more due to temperature and density than to tides and winds.

Now it is true in regard to tides, that is, the currents resulting from tide-waves are mainly littoral and local. It is not so, however, as the result of winds, which from my experience are the main sources of ocean-currents, without ignoring that from the rotation of the earth, which are therefore chiefly superficial, but capable of reaching a considerable depth where the water is deep enough, even to 50 and more fathoms, with no greater surface-movement than from three-tenths to five-tenths, or half a knot per hour, as I have on several occasions experienced from a perfect stillness of the sea from the surface down to the greatest depths in perfect calm weather, but which was set in motion in the same direction as the wind to that depth a few hours only after a 4- or 5-knot breeze had set in.

To show that Capt. Maury had mainly founded his theory upon the observations of Lieutenants Walsh and Lee, I must quote from the Report of the former as being the one most important and complete, as was supposed, in proof of the rapid undercurrent believed in by Capt. Maury, and supposed to have been confirmed by other phenomena connected with the fallacious idea of the ploughing of icebergs through fields of ice in Baffin's Bay, by the force of a mighty undercurrent*, instead of the fact of the field of ice flowing past them, by reason of the greater strength of the surface-current over the current in the depths to which the base of the bergs reached, as no doubt must be the fact in that bay or strait from the southerly drift of both into the Atlantic.

"Report of Lieut. Walsh, U.S.N., to Lieut. M. F. Maury, of the Observatory at Washington.

"The next subject to which I would refer is our investigation of the undercurrents of the ocean. I regret we had so few opportunities for the interesting experiments, but enough has been done to seem to warrant the conclusion that these undercurrents are *generally* stronger setting in various different directions than those of the surface. I am well aware there is no mode of testing their exact velocity, but that practised by myself, which I will describe, was certainly all-sufficient to show their real velocity. There may be none so rapid as that mighty ocean-river the Gulf-stream. Unfortunately the weather prevented our making these investigations in that interesting region; but in the various parts of the Atlantic in which we succeeded in these experiments, on only two occasions did we find the undercurrent of less velocity than that running in a different direction above it.

* See Physical Geography of the Sea, pp. 162 & 163.

“The following is the mode practised: the surface-current was first tried by the usual mode (a heavy iron kettle being lowered from a boat to the depth of 80 fathoms), then for the trial of the undercurrent a large chip-log, of the usual quadrantal form, the arc of it measuring full 4 feet, and heavily loaded with lead to make it sink and keep upright, was lowered by a light but strong cod-line to the depth of 126 fathoms (the length of the line); a barrega was attached as a float, a log-line fastened to this barrega, and the rate of motion to this float, as measured by this log-line and the glass, and the direction as shown by a compass, were assumed as the velocity and set of the undercurrent. No allowance was made for the drag of the barrega, *which was always in a different direction from the surface-current*. It was wonderful, indeed, to see this barrega move off against wind and sea and surface-current at the rate of over one knot an hour, as was generally the case, and on one occasion as much as $1\frac{3}{4}$ knot. The men in the boat could not repress exclamations of surprise, for it really appeared as if some monster of the deep had hold of the weight below, and was walking off with it.”

It is therefore quite evident that Capt. Maury adopted Lieut. Walsh's identical words and views as the sound solution of the experiments, viz. that a great oceanic undercurrent circulation existed as a counterpoise to the disturbed densities arising from temperature and salinity.

Lieut. Walsh next cites from the log several instances of the experiments, viz. at six positions in the Atlantic, between $24^{\circ} 43'$ North and $65^{\circ} 2'$ West, and $33^{\circ} 58'$ North and 72° West, in which the weighted chip-log was lowered to 126 fathoms in each position, to test the undercurrent at that depth, as erroneously supposed. But in fact it was merely giving a more correct indication of the surface-current than that resulting from the iron kettle in 80 fathoms, with a large boat as its float, under the erroneous impression that the iron kettle would be in still water at that depth, and that it would retain the boat stationary as if anchored to the bottom; this, too, against wind and sea. It is, however, evident that the kettle-and-boat experiment could only show a vitiated result, a diminished surface-current to that actually existing.

The kettle-and-boat experiment were only used once, however, at the last position, in connexion with the large chip-log lowered down to 126 fathoms, which Lieut. Walsh regrets, by saying, “which it would have been better to have always done.”

Now it must be quite evident from what I have before shown, from my experiments for testing surface and undercurrents, or from the diagram referred to, p. 537, how the rate of descending surface-currents, and of any undercurrent, can be correctly ascertained, if existing as an appreciable fact, although Lieut. Walsh did not then know “of any means of doing so correctly;” that, therefore, the float to the large 4-foot diameter chip-log, lowered down to 126 fathoms, would naturally appear to run to windward of the heavy boat attached to the iron kettle of less dimensions.

than the former, and therefore of less resistance to the drag of the boat by the wind, sea, and surface-current, even if both kettle and chip-log had reached the region of perfectly still water.

But if, as may have been probable, the kettle was still in a portion of the surface-current, and the chip-log in about 50 fathoms lower down was in the still regions, or even a more diminished rate of the surface-current, the float of the latter would more rapidly separate from the boat in the opposite direction to the surface-current, and thus appear to be marvellously dragged by an undercurrent against wind and sea and surface-current—that is, against or opposite to the boat's natural drift. Now, as Lieut. Walsh notices that the weather was too rough for attempting deep soundings, except on the 14th of May, we must infer that there was sufficient wind and sea to cause considerable drift to the boat; but he does not notice the direction of the wind.

Therefore, as there was no fixed object as a point of comparison sufficiently exact in the last experiment, when the kettle was used, much less in the others, when only the chip-log was used, and with a compass bearing from a drifting boat for ascertaining the presumed direction of the undercurrent, even the true direction of the surface-current cannot be depended upon by reversing the direction he has given for the undercurrent (this is, by assuming that the surface-current ran E.N.E. $1\frac{1}{2}$ knot when he gives the undercurrent as setting W.S.W. $1\frac{1}{2}$ knot), since there were so many sources of error vitiating the results. A fixed object for reference can always be obtained in any depth by a 20-lb. lead and sufficient twine, and a light float attached when it has reached the bottom, as I have long since shown and recommended *as a necessity* in all such delicate experiments in mid-ocean or elsewhere. The following are the results at the six positions given by Lieut. Walsh:—

Date.	Lat.	Long.	Depth.	Rate.	Direction.	Temperature, surface.	Fathoms, 100.
May 11th ...	24° 43'	65° 2'	fms. 126	knot. 1	W.S.W.	77° 3'	73·5
May 12th ...	24 55	64 43	126	$1\frac{1}{2}$	S.E.	75	69
May 13th ...	26 42	64 4	126	$1\frac{1}{2}$	W.S.W.	77·5	74·5
May 14th ...	26 46	63 53	126	$1\frac{1}{2}$	N. by E.	77	
May 18th ...	30 06	67 56	126	$\frac{1}{2}$	N.E.	70	65
May 20th ...	33 58	72 00	126	1	W.N.W.	71	67

It is therefore only the rate as given above for the undercurrents that can be relied upon as the rates of a surface-current that really existed in those positions, and which from the high mean temperature for the whole six, of 74°·6, and of 77°·3 at three of the positions, as the surface-temperature for the month of May, and of 73·5 and 74·5 in a depth of 100 fathoms at two of them, would seem to show it to have been a continuation of a portion of the trade-wind or equatorial current, its easterly portion running outside of the West-India Islands, but somewhat checked, and even perhaps at times

diverted by the local winds; for the power of winds to divert even the "mighty Gulf-stream" of $3\frac{1}{2}$ knots is shown in the 'Notes on the Gulf-stream,' by A. D. Bache, Superintendent of the United States Coast Survey, who shows it to be driven sometimes out of its usual course fully thirty miles by N.W. and westerly gales.

In concluding these remarks upon the errors regarding the undercurrent theory, I feel that it is due to our distinguished transatlantic hydrographers and geographers, that as theirs were the pioneer efforts in such investigations on a large scale, it was natural that they should have been defective, from the little attention given to such researches previously. But it is necessary that these errors should now be well understood, and shown to have arisen from a fallacious estimate of the experiments, that the philosophical naturalist and physicist be no longer misguided by them, and thus attribute so grand and large an influence to undercurrent, as erroneously shown by the experiments of Lieut. Walsh and Lee, and as the assumed necessary result of the small difference of density between one part of the ocean and another; for surface-current circulation and return can and, indeed, must tend largely to restore it, aided by the rain and river supply of fresh water met with in its circulation and return. This is even shown to be so under the equator, from the large African rivers and also from the Amazon and others joining the equatorial current, from the elaborate investigations (the twenty years' researches) of the late Dr. Forchhammer, as summarized in his most interesting and valuable discussion of these analyses in his paper in the Philosophical Transactions for 1865, wherein the analyses and temperatures of the sea-water are given from all parts of the globe, and a most remarkable and able deduction of the surface-currents from them. But the learned Doctor, misguided, no doubt, by the supposed existence of the great undercurrent movement in the Atlantic as propounded by Capt. Maury, and also by the misunderstanding of the facts and the incompleteness of the observations for correctly ascertaining the conditions existing between the Baltic and German Ocean, and so, as a philosophical physicist, thus misled, was induced to ascribe a greater influence to undercurrent circulation than to superficial, as a means of restoring the equilibrium from reduced or increased densities. I have before admitted that where two currents meet, such as the Polar and Gulf-stream, both strong in force and of great difference in density or temperature, and in directions nearly at right angles such as these two, an undercurrent or intermediate current of appreciable amount may exist.

That denser water will intermix with lighter water in its deeper portion, when they meet and when depths are equal and difference of the densities great, as between pure fresh and sea-water, I am aware from my experience at the mouths of large rivers.

This is a fact experienced every year at the Damietta and Rosetta mouths of the Nile for five or six weeks, during the lowest condition of the Nile, when there is only a surface-outflow there; and it extends five or six miles

within their embouchures, as I first ascertained when carrying out my current tests in that river; brackish water was consequently found in the lower depths, percolation of sea-water through the sandy substrata no doubt then partly accounting for its brackish condition. But eddy surface-currents along the sides of the river, under the influence of the prevailing N.W. winds blowing directly into the mouths, no doubt also assist the intermixture as far as it goes—just as the return-current down the European coast is diluted and intermixed in its general and superficial density from the rains and rivers of the north, and thus tends to restore the lost freshness of the equatorial or trade-wind and Gulf-stream currents, as the tropical rivers and rains tend to restore the loss in the low latitudes: thus condensation from evaporation and redilution by surface-currents are throughout mainly maintaining the equilibrium. Dr. Forchhammer shows that this lighter density or dilution of the encircling superficial waters from the equator commences from the American rivers from the parallel a little north of the Bermudas, and that it exists all along the European coast and again on the African coast from the African rivers; and he has shown that the effect of the La Plata is found 900 miles from its mouth. The fact I have given of the condition at the Nile's mouths at certain seasons is an extreme case, quite in accordance with the great undercurrent theorists' views, and I mention it as a fact of interest to them. But nevertheless I believe, from my own experience, and from the facts to be gathered from Dr. Forchhammer's elaborate researches into the temperatures and saline densities, that as it is not an appreciable and measurable movement as an undercurrent at the Nile or Dardanelles, and only chemically testable by the tongue or hydrometer, so are there no great mechanical and appreciable movements as undercurrents in the ocean as a necessary result of the very slight difference in the densities of one part of the ocean and another. Nevertheless a complete investigation into the phenomena of ocean-currents is a most desirable operation, and can be so easily accomplished on the plan I have found so practicable and easy, and recommended several years ago for adoption by all scientific captains when crossing the great oceans, especially when calms detain them and favour the experiment, without fear of the results being confused or mistaken; for then only should it be carried out where there are great depths and where strong surface-currents exist.

XII. "On the Physical Principles concerned in the passage of Blood-corpuscles through the Walls of the Vessels." By RICHARD NORRIS, M.D., Professor of Physiology, Queen's College, Birmingham. Communicated by Dr. SHARPEY, Sec. R.S. Received June 12, 1871.

In the year 1846 my much-lamented teacher, Dr. Augustus Waller, published in the Philosophical Magazine two able papers relating to the

perforation of the capillaries by the morphological elements of the blood, viz. the red and white corpuscles.

These observations attracted little attention till the year 1867, when the facts made known by Dr. Waller were rediscovered by Professor Cohnheim, of Berlin.

Since the publication of Cohnheim's researches very considerable interest has been taken in the subject, and the experiments have been repeated and the facts corroborated by eminent physiologists and pathologists in all parts of the world.

On a careful consideration of the hypotheses which have been propounded by Waller, Cohnheim, Stricker, Bastian, and Caton, to account for the curious phenomena in question, it will be found that all these hypotheses fall short in one important particular, inasmuch as they afford no explanation whatever of by far the most singular part of the process, viz. the fact that the apertures through which the corpuscles pass again close up and become invisible. The question, indeed, is not so much how the corpuscles get out, as how they get out without leaving any permanent trace of the apertures through which they have so recently passed, and which were so palpable during the period of transit.

Before proceeding to elaborate my own views, it may be well to restate succinctly the various points upon which observers are agreed.

1st. Both white and red corpuscles pass out of the vessels through apertures which can neither be seen before their ingress into or egress from the vessel wall, but only during the period of transit.

2nd. An essential and primary step in the process is, that the corpuscles shall adhere or, more properly, cohere to the wall of the vessel.

3rd. These cohering corpuscles shall subsequently be subjected to pressure from within.

With these conditions fully before our minds, we will proceed to inquire if in physics we can find the analogue of these seemingly mysterious phenomena.

In the first place, this phenomenon of the passage of bodies through films or membranes is by no means confined to the capillary walls, the same thing has been observed in nucleated blood-corpuscles, such, for example, as those of the frog. In these cases no rupture or aperture of exit has been discovered.

It is obvious that the escape of the nucleus from its capsule without rupture, and the passage of the entire blood-corpuscle through the capillary wall without rupture, are phenomena of the same class; and the explanation which will suffice to clear up the one, will also apply with equal force to the other.

As a matter of fact, it will be admitted that we can form no *à priori* conception of one form-retaining body passing through another without either rupturing it or distending certain holes or pores which it may already possess. This, however, is just one of those cases in which con-

ceivability is no test whatever of possibility. To comprehend these phenomena it is necessary to bear in mind the ultimate constitution of the animal membranes, which form alike the capsules of the corpuscles and the parietes of the capillaries*. All the membranes which enter into the animal body may, from a physical point of view, be divided into two orders, —the very fine structureless homogeneous films which must be regarded as simple cohesion-membranes, in contradistinction to the second order of coarser membranes, to which certain mechanical arrangements are super-added, which have the effect of increasing the strength, such, for example, as structure, the result of interlacing fibres; in films of collodion, gelatine, albumen, india-rubber, and soap we have examples of the first class of membrane. It is with this class that we are now concerned, and these are susceptible of two states, the fixed or rigid condition, and the contractile or elastic state, dependent upon the presence of the principle of "flow," which principle may be operative in every shade and degree, from perfect liquidity to absolute rigidity.

It will be sufficient to state here that the more colloid and plastic those membranes are, or, in other words, the more they approximate in their constitution to liquids, so do they proportionately cease to obey exclusively the laws of rigid bodies, and begin to exhibit intermediate properties or qualities, some of which belong to solids and others to liquids.

We may take the soap-film as the best illustration we can find on a large scale of the class of homogeneous cohesion-films, possessing in the greatest perfection this principle of "flow," and as exhibiting to the fullest extent phenomena which I have generalized under the term progressive cohesive attraction†.

By the study of the soap-film we may acquire a knowledge of many

* The parietes of the capillaries are held by modern histologists to consist of protoplasm, a substance which is universally considered to be of a viscid semiliquid nature, and in which it is easy to demonstrate the presence of the property of flowing within certain limits.

† The term progressive cohesion is here used in contrast with that operation of cohesion which simply maintains contact between the particles of two like or unlike substances or bodies, and which, when it occurs between the particles of unlike bodies, is called adhesion. The attraction of cohesion evidently operates for some distance beyond the atom or particle, so that actual contact is not essential to its display. When two small globes of mercury, or of any other liquid, are made to touch at one point, they merge, as is well known, with great rapidity into each other, and the materials which compose them become arranged around a common centre, that is to say, one larger sphere results. The mode of union of these two spheres is clearly a progressive one, the particles nearest to those in actual contact being the next to come into contact, and so on, until the globes become intimately united. In the presence of gravitation there can be no mass-attraction between the two globules.

Again, when a solid is partially immersed in a liquid having a cohesive affinity for it, *e. g.* a sheet of glass, the liquid, as is well known, rises considerably above the water-level. This shows that with unlike bodies the action extends beyond contact, and is progressive in its operation from one line or row of particles to the next above. This term therefore includes all effects of cohesion which arise from and display its opera-

of the laws which are operative in connexion with delicate colloidal films in general.

The steps, for example, in the production of an ordinary soap-sphere are very remarkable, as exemplifying the power which these films possess, under the influence of progressive cohesion, to perfect any absence of continuity which may exist in their structure.

The first essential in the process of forming a soap-sphere is the production upon the mouth of the pipe-bowl of a film stretching evenly across from every point of the circumference.

The production of this film is a far more complex operation than is generally supposed.

If for the pipe-bowl we substitute a ring having a diameter of from 12 to 18 inches, we are enabled to watch, as the process proceeds, the manner in which the film is formed.

Having submerged the ring in a solution of soap, we observe, as we gradually raise it out, that its circumference brings up from the liquid a band-like film of a cylindrical or tubular form which is attached to the ring above and the liquid below; raising up the ring still higher, we find that this annular film contracts in diameter at every part except at its attachment to the circumference of the ring, which is of course fixed. This quality of the film to contract between opposing points of extension causes it to take on the shape of an inverted cone with curved sides, the convexities of which are directed inwards. The tendency to assume the inverted-cone shape is further assisted by the fact that the film, in contracting, travels inwards upon the surface of the solution towards a central point, so that from the ring downwards to the surface of the solution the diameter of the tubular film is continually decreasing. The shortest diameter is not, however, immediately upon the surface of the liquid, but at a little distance from it; and consequently, as the contraction proceeds, it will be at this spot that the union of the sides of the film and the separation will take place. This arises from the fact that this is the weakest point of tension between the ring and the liquid, and therefore the one in which circumferential contraction can take place with the greatest ease and effect. Thus we see that the tubular film which we have raised really becomes constricted into two portions,—an upper portion, which immediately contracts into a plane surface upon the ring, and a smaller and lower portion, which, in consequence of including air, becomes a hemisphere and remains attached to the surface of the solution. If, having formed such a film upon a ring or pipe-bowl, we proceed to blow down upon it, we distend it into a sphere; but it is obvious that until the sphere is detached

tion beyond the line or boundary of actual contact. All capillary phenomena may be regarded as due to this progressive action of cohesion operating at one and the same time in diverse directions.

there exists a free opening into it at its upper part, which becomes suddenly sealed up by cohesion of the sides of the film at the moment preceding detachment; and this detachment is seen to be a repetition of what takes place in the formation of the primary film.

The next point to which I would draw attention is the power possessed by these films to repair breaches of continuity that may be made in them subsequently to their formation. If any rigid body be wetted, it is quite possible to thrust it through one of these films, move it about, and again withdraw it without interfering with the integrity of the structure, as may be proved by passing a smooth bulbous rod of glass through the film. It is not, however, essential that the body should be either smooth or regular, for the same thing may be done with the naked fist and arm.

I have demonstrated elsewhere that the blood-corpuscles undergo a mode of aggregation in obedience to progressive mutual attraction in precisely the same fashion as soap-spheres,—that is to say, if they touch at any one point they gradually, by the operation of double cohesion (capillary attraction), convert each other into polyhedral-shaped bodies*. If we wet any smooth rigid surface and allow one point in the circumference of a bubble to impinge against it, we find that it becomes so drawn down to the plate in every direction, from this point as a centre, as to take on a hemispherical form. But if for the rigid surface we substitute a delicate flowing film, such as the soap-film, and allow the bubble to come in contact with it at one point, taking care that there is a free supply of liquid upon its exterior at this point, we observe that the result is different. In this case the soap-sphere takes on the form of two watch-glasses in apposition at their edges, one of the curves being present on each side of the film. The soap-sphere has, in fact, penetrated the film, and arranged itself so that half is on one side and half on the other.

Now this is precisely analogous to what takes place with the capillary when the corpuscle has entered into cohesion with its wall; “a protuberance is seen on the outer surface.”

If we can subject this soap-sphere to pressure on one side only, we shall cause it to protrude through the film still further; this we can do by forming one sphere within another. This inner sphere protrudes more than in the case of the simple film. That there is pressure within a bubble may be known by the fact that, if left with an aperture in it, it will gradually force out the contained air and become again a simple film by its strong cohesive tendency.

Further, it will be seen that we can with the greatest ease separate these cohering spheres, bringing them bodily through the film without injury to one or the other; and this may be taken as a parallel case to the passage of the nucleus through the capsule of the corpuscle, and of the corpuscle itself through the capillary wall.

I have previously shown that the corpuscles are amenable to the same

* Proceedings of Royal Society, vol. xvii. p. 429.

laws as the soap-spheres, and we have only to infer that they bear the same relation to the capillary walls as these spheres and films bear to each other. The margin of speculation is therefore small.

In the case of the corpuscles this relation is of course only seen under abnormal conditions, simply because it is a physical law which in the normal working of the animal economy required to be antagonized.

It must also be observed that it is only under certain conditions that the soap-spheres attract each other, or are attracted by rigid surfaces or plastic films. This occurs only when free liquid is cohering to their surfaces. If before bringing them into contact we allow the soap-film and sphere to become moderately dry, they will not attract each other, but the former will support the latter as a perfect sphere instead of drawing it down by progressive cohesion and arranging it halfway through itself.

Just so with the corpuscles; they do not unite either with each other or with the capillary wall, unless their normal osmotic relations are disturbed, the exosmotic current setting in excessively when their external surfaces become coated with content-matter, and they become instantly attractive of each other, of the capillary wall or glass slide, as the case may be.

In the paper before referred to, "On the Laws concerned in the Aggregation of the Blood-corpuscles," I have given numerous examples of the operation of progressive cohesive attraction; but in this place I wish to call attention to the demonstration there given of its relation to plane surfaces.

Taking this experiment as a starting-point, we will extend the consideration to surfaces of a different character. In the first place, we find that this law continues to operate with great facility in connexion with surfaces curved in one direction only, whether the surface used be convex or concave; in both cases the film of paper or collodion applies itself evenly to the surface in the gradual progressive manner before explained.

If, however, for surfaces curved in one direction only, we substitute such as are curved in all directions (for example, the outer or convex, or the inner or concave surfaces of a hollow sphere), we find ourselves confronted with a new set of difficulties, out of which we may evolve the statement that, for any film to apply itself evenly and regularly to either the convex or concave surface of a sphere under the influence of progressive attraction, it is necessary that the film should be, in several particulars of its constitution, very different from the class of films by means of which we have been able to illustrate the three preceding experiments.

If, by way of illustration, we apply a film of wetted collodion or fine cambric paper to the sphere, so that one point of the convexity of the latter may come in contact with the centre of the film, the attraction will only succeed in pulling it down to the surface of the sphere at certain points; the intermediate puckered parts are not in contact, and can by no possibility become applied. From this we see that for the film to be laid down evenly, it would be necessary that it should contract in certain parts,

that, in fact, the puckered or surplus material should be taken up. We may say, then, that any film which can adapt itself to the surface of a spherical body must possess the twofold quality of facile contraction and expansion, these qualities being controlled in their operation by progressive cohesive attraction. Such a film must be a simple colloidal cohesion-membrane in possession of the property of "flow."

Further, if we apply to a sphere a film known to possess facile properties of expansion and contraction under the influence of slight forces, such as progressive cohesive attraction, the first thing seen to occur is cohesion of the film to the sphere at the point of contact; and from this point as a centre of operation the film proceeds to apply itself gradually in all directions, so that the sphere becomes coated or covered evenly by it: this process goes on till such time as the attraction becomes balanced or fully antagonized by the elasticity of the film, that is to say, the attraction is only powerful enough to stretch the film to a certain extent; so that if the rigid object be fixed, as is the case with the glass bulb when held immovably, we get a flattened form of the film. A sufficient degree of attachment of the film to the bulb has taken place to stretch the former backwards out of its normal plane. If, now, we push the bulb further forwards, the film still continues to apply itself to its surface, and having reached the equatorial line of the sphere, descends on the opposite hemisphere till the bulb is completely coated. But it will be said the bulb does not then really produce an infraction of the film, but merely attracts it down to its surface, and in so doing stretches it, so that it is in reality a new conformation of the film and not a breach of its continuity. That this is true to a certain extent there is no doubt, but it is not all the truth; for we may wipe the bulb dry after it has passed through the film without interfering with the continuity of the latter. All that appears to be necessary for these effects to display themselves is, that there should be mutual cohesion between the film and the body passing through it; for if we press against one of these delicate films with a substance which has no cohesion for it, *e. g.* a current of air or a dry soap-sphere, it simply distends the film, neither bursting it nor giving rise to an aperture in it; while in the case of a body to which the film can cohere, it would appear to be easier for the latter to allow the passage of the cohering body than to suffer distension by it, and this because it has under these conditions as great an attraction for the particles of the body as for its own particles. When the cohering body has become perfectly applied to the film, the latter, by the cohesiveness of its own particles, contracts to the greatest degree possible consistent with still maintaining its attachment to the cohering body; and this in spherical-shaped bodies leads to a condition of things in which half the body is within and half without the film or wall*; therefore the rest of the process must be

* An excellent illustration of this principle is afforded when a light india-rubber ball or balloon is suspended from a fixed point, its surface having previously been wetted with a solution of soap. When a soap-film formed upon the ring, as in the previous

accomplished by pressure from within. It is easy to see that the manner or degree to which the corpuscle or body coheres to the film will determine very materially the method of its transmission.

All, then, that is essential for a rigid or plastic body to pass through a colloid film is :—1st, an intimate power of cohesion, either mediately or immediately, between the film and the body ; 2nd, a certain amount of pressure from within ; 3rd, power in the substance of the film to cohere to the surface of the body (or to some intermediate matter which already coheres to the surface) during its passage ; 4th, cohesive plasticity of the particles of the material of which the film itself is composed, so that the breach in it may again become reunited as it descends upon the opposite surface of the body which is being extruded.

It is quite remarkable to how great an extent these conditions appear to be complied with in the passage of the white corpuscle through the capillary wall, as affirmed by independent observers.

In the factitious examples by which I have sought to illustrate these effects, the film moves over the body, or the body through the film, by virtue of the intermediate agency of the solution which has cohesive attraction for both ; and the film does not rupture, because, while the body is travelling through, it can continue to cohere till such time as it is brought again into contact with its own particles at the opposite pole of the extruded body.

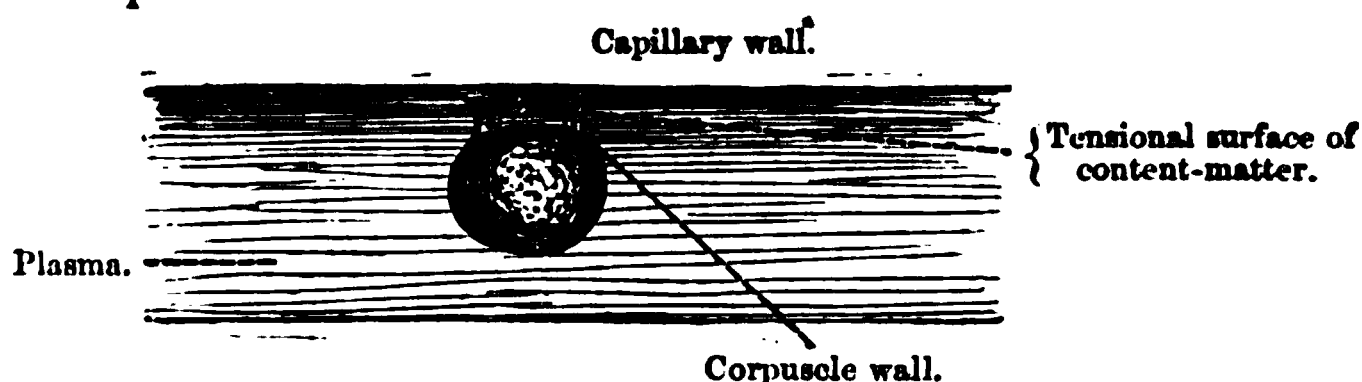
Theoretically, as it leaves the sphere or protruding body, the aperture should gradually narrow to absolute union at a focal point, or, according to the laying-down view, having resealed itself from the bulb ; practically, however, I find that the film rarely leaves the bulb or sphere without forming on it a small hemispherical bubble, which is large in the ratio of the rapidity with which the detachment is effected.

If detached with very great care, the bubble is exceedingly small ; but I could not succeed with a spherical bulb in getting rid of it altogether ; with a more conical bulb, however, this was readily effected. In the case of the sphere, the film is in reality drawn out into a little neck, as in the other examples in which continuity is effected ; and this neck is pulled into two, and both parts cohering at the point of severance, we get on the one side the perfected film, and on the other a small enclosure of air which takes on the hemispherical form. This is owing to the annular contraction of the tubular part. If the body were small or less spherical, or the film a trifle more rigid, this would not occur. I find, in fact, by experiment that smaller bodies, more conical in their termination, do not do this, but draw out a kind of streak of solution as they leave the film—a fact I have often observed with the white corpuscles. In

experiments, is brought into contact with one point of its convexity, the ball is at once drawn into the film as far as its equator, and is compelled to retain this position in opposition to the force of gravity. This is the exact converse of the case of the fixed bulb, in which the attraction is satisfied at the expense of the expansibility of the film.

this case the film is brought to a focus upon the body, and not at a slight distance from it; so that either or both these modes might obtain with the corpuscle. In some cases the streak of solution is absent. The method of sealing, which leaves behind a portion of the film, is probably a necessity of every case of repair of continuity, with the exception of that of transmission of a foreign body through a film.

In the case of the blood-corpuscle it would not appear that the capillary wall became applied over the surface of the corpuscle to any great extent, but that, having effected cohesion, it becomes easier for the capillary wall to give way and glide over the corpuscle than to be distended by it; and this is effected much slower than in the case of the factitious examples which I have placed before you. For capillarity to come into play, the presence on the exterior of the corpuscle of another and cohesively dissimilar liquid to the liquor sanguinis is required; and this we obtain by the outward passage, under the influence of osmosis, of the content-matter (hæmoglobin) of the corpuscle; a magnified view of the relations present may be thus represented:—



We may conclude in the appropriate words of Herbert Spencer:—
 “We have in these colloids, of which organisms are mainly composed, just the required compromise between fluidity and solidity; they cannot be reduced to the unduly mobile conditions of liquid and gas, and yet they do not assume the unduly fixed condition usually characterizing solids; the absence of power to unite together in polar arrangement leaves their atoms with a certain freedom of relative movement, which makes them sensitive to small forces, and produces plasticity in the aggregates composed of them.”—
Principles of Biology, p. 23.

The Society adjourned over the Long Vacation to Thursday, November 16th.

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END OF THE NINETEENTH VOLUME.



OBITUARY NOTICES OF FELLOWS DECEASED*.

JAMES DAVID FORBES† was the youngest son of Sir William Forbes of Pitsligo, who was descended from the ancient family of the Forbes of Monymusk, on the banks of the Don, and was head of the well-known banking-house long established in the Parliament Square of Edinburgh. The mother of James Forbes was the only child and heiress of John Belches, afterwards Sir John Stewart, a cadet of the old house of Invermay. The early death of Lady Forbes, while her youngest son was a child scarce two years old, cast a sobering shade over his early years, and indeed coloured his whole life and character. His father idolized the child, as the last legacy of her whom he had lost. He retired from Edinburgh, and lived a secluded life, surrounded by his young family, in his country place of Colinton. There were spent James Forbes's childhood and boyhood. The teaching he got was of the most private, even desultory kind. Fear for the boy's health made his father nervously anxious lest he should overwork himself. So his education was left entirely to his sisters' governess, and to occasional lessons from the neighbouring village schoolmaster, a worthy man, to whom his distinguished pupil remained ever afterwards sincerely attached. But though lessons were easy, his mind was active, and by his twelfth year the natural bias towards physical knowledge was manifesting itself. Already his head was busy with mechanical contrivances,—a new velocipedometer, an anemometer, a metal quadrant made by his own hands for astronomical purposes. At the same time he was devouring every scientific book he could lay hands on, from the 'Nautical Almanac' to Woodhouse's 'Astronomy.' But all this devotion to science was kept strictly secret; he laboured at it in private, and said nothing, for his father would not have smiled on such pursuits. He would have objected to them both as too serious a tax on the young brain, and also as likely to turn him from the dry studies of the bar, for which he was destined. His own wish was to take orders in the English Church, to which he was strongly attached; but from this aim he was, not without reluctance, withdrawn by his father's expressed wish that he should study for the Scottish bar. This accordingly, from his fifteenth to his twenty-first year, was his ostensible purpose, but his heart was all the while turning secretly aside to the for-

* Note to a passage in the Biography of M. Foucault, Proceedings of the Royal Society, vol. xvii.

It appears from a notice in the Report of the British Association for the Promotion of Science for 1843, that Dr. Joule had made an experiment demonstrating the conversion of work into heat, very similar to the experiment by M. Foucault described at the end of page lxxxiii.

† This notice of Professor Forbes, except what more especially refers to his scientific work, has been taken, with abridgement, but without other alteration in matter or language, from an inaugural address delivered by his distinguished successor, Principal Shairp, at the opening of the Academical Session of the United College, St. Andrew's, in 1869.

bidden paths of science. When he was only sixteen he began to keep a record, entitled "Ideas of Inventions;" because, as he expressed it, so many more ideas, chiefly mechanical, occurred to him than he could possibly execute; and, in order to remember these, he "resolved to write them down, taking most special care to distinguish between what were original and the parts which were borrowed." About the same time he began a journal of personal observations in astronomy, which he continued, with scarce an interruption, for seven years. He then also began to keep a meteorological journal, in which he recorded all his observations on the temperature and state of the atmosphere, with speculations as to their causes. The two last, especially, were continued for years. These were his real and earliest educators in science. By these he trained himself to be the patient and accurate observer which he ultimately became.

Thus entirely home-trained, and, indeed, self-educated, young Forbes entered Edinburgh University in Session 1825-26, and joined the Classes of Latin and Chemistry. About the close of his first year at College he entered on another phase of his self-education, which was destined to have important results. He commenced an anonymous correspondence on scientific matters with the late Sir David Brewster. The lad of seventeen wrote to the then renowned man of science, offering him for insertion in his well-known 'Philosophical Journal' a paper containing an attempt to account for the apparently infinite number of the stars. The paper was not only inserted, but the following words were annexed to it: "We should be glad to hear again from the author of this article, and, if possible, learn his address." The first part of this request was readily complied with. For several years hardly a Number of the journal appeared without some paper either of original observation, experiment, or cautious speculation from the young votary of science. The latter part of the request he was more slow to meet; all the communications still bore the original signature, or " Δ "—a disguise which the author's modesty induced him to assume, and which Sir David tried to pierce for some years in vain. Those who are best versed in these subjects will, I believe, most appreciate the natural insight, careful toil, and patient observation embodied in those papers, written at seventeen, with none to help or consult with, indeed, in the utmost secrecy. Towards the end of 1826 the young student's College course was interrupted (but his philosophical correspondence was not) by a year spent in Italy with his father and family. With his passion for science in no degree abated, he entered into all that Italy contains to feed the imaginative and historic mind, almost as fully as if he had been exclusively a scholar or man of letters—so early appeared that fine blending of literary taste with scientific exactness which in after years lent to his lectures and his writings so great a charm. The 'Philosophical Journal' contained some fruits of his Italian experiences, in what Sir David styles " Δ 's very excellent set of observations," made at Rome, on the climate of Naples, and on the phenomena of Mount Vesuvius. These three papers appearing in successive Numbers of the Journal,

called forth much attention from scientific men. On his return home to Colinton, in the autumn of 1827, his diary records that he is pleased "to see Brewster's journal, and read the articles of mine he inserted in October last on the apparent number of the stars, the heats and colds of last year, and elements of the lunar eclipse, together with all the other papers which I have sent, inserted, or favourably noticed." In the correspondence with Brewster, the disguise of Δ was preserved to the close of 1828. Many and curious were the attempts Sir David made to pierce behind the mask and see the real face of his unknown contributor. In one of his letters he says :—" We who have begun our downward course look anxiously for some rising stars ; but, excepting yourself and Mr. F., I know of no young men who are likely to extend the boundaries of science." Who was Mr. F.? Was he the same as the unknown Δ ? When at length the mask was withdrawn, the welcome which the elder philosopher gives to his younger fellow-labourer is highly characteristic. He expresses his joy "that Scotland possesses one young man capable of pursuing science with the ardour and talent of Δ , and that he belonged to a family for which he had so much esteem and affection." He then goes on to advise him to allow no professional duties to turn him from science ; he would find in it a solace and delight amid the bustle and vexations of life.

Though thus early launched into original inquiries, James Forbes was still only a Student of Arts in Edinburgh University. The excitement of young and successful authorship seems never for a moment to have turned his head, or to have made him bate one jot the patient industry by which only college classes can be turned to account. In the Moral Philosophy Class, which he attended after returning from the Continent, we find him preparing with great labour an essay of sixty large quarto pages for Professor Wilson. The essay was on the influence and advantages of the study of astronomy on the mind, and it was accompanied by scientific illustrations and notes. This and other essays of the young physicist so far commended themselves to the Professor of Ethics, that at the close of the Session he made him Medalist in the Class, and ever afterwards received him to intimate friendship.

The month before James Forbes entered the Class of Natural Philosophy an event happened which deeply impressed him. His father was removed by death, and this was soon to be followed by the breaking of the old home at Colinton House. This bereavement formed a turning-point in his life, and deepened his already religious character. Solemnized, yet braced, he entered on the Natural Philosophy Class, then taught by the celebrated Sir John Leslie, in his later years. In the subjects of that Class lay his own specialty, but he had never received one word of mathematical instruction from any one, all his mathematics were entirely self-acquired. Yet, notwithstanding this disadvantage, in the competitions with students who had passed through a regular Mathematical Course he held the first place, and closed the Session by easily bearing off the highest honours. While he was

a Student in the Natural Philosophy Class he had the honour of being proposed by Sir David Brewster as a Member of the Royal Society of Edinburgh before he had reached his twentieth birthday.

Though his College course may be said to have been now completed, he took yet another Session, attending Sir John Leslie's Class, and the Chemistry Class of Dr. Hope each for the second time, and combining with these two the study of the Law. At the close of his College life, in April 1830, he looks back on it, in his diary, "with peculiar satisfaction, as comprising the happiest period of his life." That summer (1830) he passed his Law trials, and put on his advocate's gown, but never wore it. To his great joy, having obtained the full concurrence of his family and friends, he cast law for ever behind him, and, content with a small competence, gave himself unreservedly to science. This resolution was not taken hurriedly. But his mind once made up never faltered. At the beginning of next winter he says:—"I now enter on the delightful and engrossing studies, which have now, blessed be God, become my principal and legitimate object, untrammelled by jarring occupations and conscientious scruples. He then gave himself to closer study of the higher mathematics, and at the same time began those experiments on heat which were afterwards to result in one of his best scientific achievements. A hint of his future destiny was at this time given him. He had happened to go to hear Sir John Leslie's opening lecture in that Professor's last Session but one, 1830-31. At the close of the lecture Sir John, who had never before admitted his promising Student to any special intimacy, sent for him, and, after asking him about his own studies, told him that when he (Sir John) proposed going to the East last summer, he had thought of getting him (James Forbes) to officiate for him, but was afraid the public might think him too young. He then broke off abruptly. In exactly two years from this time young Mr. Forbes, who had gone to the Continent on a long scientific tour, was recalled by the news of Sir John Leslie's death, and that his friends in Scotland had given in his name as a Candidate for the vacant Chair. Then ensued a contest, not the least memorable of those many contests of the same kind by which Edinburgh has made itself conspicuous. Two things made this one especially warm, and even painful. By one of those strange turns in men's destiny, Mr. Forbes's chief opponent was his friend and patron, Sir David Brewster, now almost a veteran in the army of science. Political feeling, too, was added. It was the era of the Reform Bill, and party spirit, then running high in Scotland, as elsewhere, entered as an element of the contest even more than it usually does. Mr. Forbes, though only twenty-three, was elected by a very decisive majority; and, however it may have appeared at the time, the result, we know, justified the wisdom of the choice. It is pleasing to be assured that, whatever passing feeling the contest may have awakened, the old intimacy was soon renewed, and the friendship so honourable to both

these distinguished men continued unimpaired till the close of their lives.

He thus entered on a Professorate of seven-and-twenty years, carried on with an energy and success rarely equalled, never surpassed. He began to teach the Natural Philosophy Class in Session 1833-34, when he was only four-and-twenty, yet from the very first he rivetted the attention of one of the largest and most distinguished classes of students that Edinburgh University ever contained. One great feature of his teaching, as I have always understood, was that while it stimulated the powers and won the admiration of his most scientific listeners, he yet made himself attractive to all students who were intelligent, though less highly gifted.

In these lectures the whole range of Natural Philosophy was gone through, and the laborious and well-sustained study required for the due performance of his professorial work had doubtless prepared him for the task, which he successfully accomplished, of writing the well-known "Dissertation on the Progress of Mathematical and Physical Science, principally from 1775 to 1850," which was published in the last edition of the 'Encyclopædia Britannica.' Meanwhile, however, Professor Forbes was indefatigable in original investigation, for which he had so early shown an aptitude; and the separate titles of his contributions to Transactions of Societies and Scientific Journals, as given in the Royal Society's 'Catalogue of Scientific Papers,' amounted in 1863 to one hundred and eighteen. These embrace various subjects belonging to Physics, Meteorology, Geology, and Physical Geography. The most important of his earlier experimental investigations was that in which he succeeded in demonstrating the polarization of heat. Melloni had been pursuing an inquiry in the same direction, but had failed to obtain the result he was in quest of. Professor Forbes was more fortunate. The steps of his investigation are thus stated by himself in the dissertation above referred to.

He says:—"I have just referred to my own early experiments on the subject (which were likewise inconclusive), in order to explain that it was natural, on hearing of the application of the thermo-multiplier to measure radiant heat, that I should wish to repeat them with the new instrument. This I did in 1834. I first succeeded in proving the polarization of heat by tourmaline (which Melloni had announced did not take place), next by transmission through a bundle of very thin mica plates, inclined to the transmitted ray, and afterwards by reflexion from the multiplied surfaces of a pile of thin mica plates placed at the polarizing angle. I next succeeded in showing that polarized heat is subject to the same modifications which doubly-refracting crystallized bodies impress upon light, by suffering a beam of heat (even when quite obscure), after being polarized by transmission, to pass through a depolarizing plate of mica, the heat traversing a second mica bundle before it was received on the pile. As the plate of mica used for depolarization was made to rotate (in its own plane), the amount of heat shown by the galvanometer was found to fluctuate just as the amount of light received by the eye under similar circumstances would have done. This experiment, which, with the others just mentioned, was soon repeated and confirmed by other observers, still remains the only one proving

the double refraction of heat unaccompanied by light; and, though somewhat indirect, it will hardly be regarded by competent judges as otherwise than conclusive. Iceland spar and other doubly-refracting substances absorb invisible heat too rapidly to be used for affecting *directly* the separation of the rays, which requires a very considerable thickness of the crystal. I also succeeded in repeating Fresnel's experiment of producing circular polarization by two internal reflections. The substance used was, of course, rock-salt."

For these researches the Royal Society awarded to their author the Rumford Medal, in 1838. Taken in conjunction with the experiments of Melloni on the absorption, &c. of radiant heat, they afforded the conclusive proof of the *identity* of thermal and luminous radiations,—a fact of the very greatest consequence to the further progress of one of the most fascinating branches of physical science.

In 1842 Professor Forbes communicated to the Royal Society a paper "On the Transparency of the Atmosphere, and the Law of Extinction of the Solar Rays in passing through it," which was adopted as the Bakerian Lecture for that year, and for which in 1843 he received a Royal Medal.

Another prominent work of Forbes is that long series of observations on the Nature and Motion of Glaciers, which he pursued with intense application, and, as there is too good reason to believe, to the serious injury of his health. He had spent several vacations on the Continent, and had wandered over the mountains of Switzerland and Savoy, studying the Geology and Physical Geography of those regions; but he specially devoted the summers of 1842–44 and 1846 to the exploration of the glaciers of the Alps. Into this pursuit he threw an enthusiasm and concentration of energy which few men are capable of. In 1843 appeared his well-known work, 'Travels in the Alps,' in which he blends interesting descriptions of scenery with scientific observations and reasonings; and the fruits of his earlier labours on the glaciers are there given. Shortly before this he commenced the interesting series of sixteen "Letters on Glaciers" published in the 'Edinburgh New Philosophical Journal' from 1842 to 1851, which contain the results of his continued study; and he also set forth and discussed his views on the constitution and motion of glaciers, in an elaborate Memoir published in the Philosophical Transactions for 1846, entitled "Illustrations of the Viscous Theory of Glacier Motion."

Not contented to limit his observations to Switzerland and Savoy, he in 1851 made an excursion to Norway to study the glacier phenomena of that country, and gave an account of his work in 'The Glaciers of Norway visited in 1851,' which was published in 1853.

Professor Forbes looked on the progressive motion of a glacier as comparable to that of a plastic mass, moulding and adapting itself to the variations in the width and depth of its channel and the inclination of its bed, and moving faster in the middle than at the sides, as would happen with such a yielding substance. A view substantially similar had been previously promulgated by Bordier in 1773, the contemporary of De Saussure, and more

recently by M. Rendu, then Canon, afterwards Bishop of Annecy ; but to Forbes belongs certainly the merit of proving its general truth by careful and prolonged experimental measurements of the rate of progression of glaciers at different parts. But whilst it is plain that the ice of a moving glacier behaves in the gross like a plastic substance, there has been a question by what intestine changes or motions of its particles its change of figure is brought about or accompanied ; and later inquirers maintain that the ice yields by breaking up into minute fragments, which speedily reunite by partial melting and regelation, thus permitting of change of form in the mass. The ribboned or veined structure of glacier ice, which had been but little attended to by previous writers, was carefully studied by Forbes. Having seen that the velocity of movement increases from the sides to the middle of the glacier, he ascribed the production of the ribboned structure to "differential motion" between adjacent laminar sections of its substance, a process which has since been termed "shearing." Others have compared the phenomenon to the lamination of slaty rocks now very generally regarded as caused by pressure in a direction perpendicular to the planes of lamination.

But while there may be difference of opinion as to the physical explanation of the observed phenomena, there can be no question of Forbes's signal merit in connexion with the scientific history of glaciers ; and it is pleasing to know that it was generously acknowledged in his lifetime by a distinguished rival in the same field, who thus speaks of him* :—"The more his labours are compared with those of other observers, the more prominently does his comparative intellectual magnitude come forward. The speaker would not content himself with saying that the book of Prof. Forbes was the best book which had been written on the subject. The qualities of mind, and the physical culture invested in that excellent work, were such as to make it, in the estimation of the physical investigator at least, outweigh all other books upon the subject taken together."

While Switserland was the main region of Forbes's explorations, he did not neglect his own land. In the summer of 1845 he traversed the rugged hills of Skye, and proved that the bare scarps of the Cuchullius had been ground down by the same kind of glaciers as those which are now wearing down the gorges of the Alps.

The last important scientific labour he was permitted to undertake was on the subject of thermal conductivity. He was the first to point out—and this at a very early period of his career—the fact that the conducting-powers of the metals for electricity are approximately proportional to their conducting-powers for heat. Now, heat diminishes materially the electric conducting-power—does it also affect the thermal conductivity ? Forbes showed that (at least in the case of iron, the only metal his failing health left him strength to examine) *the conductivity for heat diminishes as the temperature increases*. Another result of the same investigation, and one

* Report of a Lecture delivered by Professor Tyndall at the Royal Institution, June 4, 1858.

of great interest and importance in modern science, is his determination (the earliest of any real value) of the *absolute conductivity* of a substance, i. e. *how much* heat passes per second per unit of surface through an iron plate of given thickness, whose faces are maintained at constant given temperatures. As a proof of the value attached by scientific men to these ingenious experiments, it is only necessary to mention that the British Association has given a grant for their repetition with the best attainable instrumental means, and for their extension to other substances than that to which Forbes was obliged to confine himself.

But on his more active work an arrest was soon to be laid. In December 1851 he was prostrated by a severe hemorrhage in the lungs, occasioned, it was thought, in part by exposure on the Alps, in part by too close application while prosecuting further experiments on heat. For two Sessions and a half he was entirely laid aside from work. In the winter of 1854 he resumed his duties, but daily lecturing was a heavy burden on his now enfeebled strength. As for exploration or continuous experimenting, all that was ended.

“Though he could not leave Edinburgh without some natural pangs, yet,” continues Principal Shairp, “it was no doubt a relief to him when he was called to assume the Principalship of this College [St. Salvador and St. Leonard’s United College, St. Andrew’s] in the beginning of Session 1859–60. The office fitted in better to his state of health, because it relieved him from the necessity of giving daily lectures, and set him more free to do his work at the hours and in the way that suited him. He was, however, far from regarding it as a sinecure, as some speak. Our late Principal was not the man to regard any post of trust as a sinecure. He came among us, no doubt, with diminished strength. But illness had not abated his mental energy. Few men felt more the appeal which the past history and present aspect of this city makes to the imagination. But though much captivated with this, he found on his arrival enough of hard matter-of-fact work ready to his hand, and into it he threw himself vigorously.”

“Those who were comparatively strangers to our late Principal observed in him a certain antique formality and reserve which they sometimes mistook for coldness. They little knew how gentle and affectionate a heart lay under that exterior—what longing for sympathy, what appreciation of confidence and frankness in others. His thoroughness in all work, his painstaking in the most ordinary college business, his patience in getting to the bottom of every minutest detail—as great, indeed, as if it had been a link in some grave discovery—these are things which only his colleagues can know.”

“His last public act, I believe, was to preside at the laying of the foundation-stone of the new College Hall Building in 1867. A few days after, he left St. Andrew’s not to return. The sequel in its outward details (that winter abroad, the return to Clifton, and the close) would be too painful to dwell on. But while his body was reduced to the last stage of

weakness, his mind remained self-controlled, unclouded, and peaceful to the end. He departed on the last day of the departing year (1868), upheld by humble faith in Him to whom long since he had committed himself."

Professor Forbes was a Vice-President of the Royal Society of Edinburgh, and a Corresponding Member of the French Institute. His election into the Royal Society is dated June 7, 1832.

JOHANN EVANGELISTA PURKINJE was born in the town of Libochowitz, near Leitmeritz, in Bohemia, on the 17th of December, 1787. He obtained the first rudiments of education in the school of Libochowitz. He then went to Nicholsburg in Moravia, where he passed through the normal school and Gymnasium with credit, and entered the Order of the Piarists with the intention of becoming a teacher. After a noviciate of one year at Kaltwasser in Moravia, he was sent to Stráznic in Hungary as teacher in the Gymnasium of that place. In 1805 he began to study French and Italian, and also the language and literature of Bohemia. In the following year, while officiating as teacher in the normal school at Leutomischlin in Bohemia, he turned his attention to the writings of the German philosophers, and especially to those of Fichte. These pursuits opened out to him the prospect of a higher intellectual culture, which he felt to be within his reach. He accordingly quitted the school and became a student in the University of Prague, and supported himself by taking pupils. After some hesitation, he adopted the career of medicine. For two years he pursued his studies in the Anatomical Institute under Dr. Ilg, and for two more in the surgical division of the hospital under Dr. Fritz, by whom he was greatly esteemed. During this time he derived the means of living from the family of Baron Hildebrandt, to whose son he had been tutor. In 1818 he graduated as M.D., the title of his inaugural dissertation being, "Contributions to our Knowledge of Subjective Vision." In this dissertation he explained how some properties, and even some structural relations of the eye, can be investigated by means partly psychological, partly physiological, which otherwise require for their discovery the most minute microscopical examination. This essay decided his line of research; it opened a new world to his fellow-labourers, and won for himself the approbation of Göthe, who was engaged in similar pursuits. Shortly afterwards he became assistant to the Professors of Anatomy and Physiology, Ilg and Rottenberger. In 1820 Purkinje published in the '*Medicinische Jahrbücher des österreichischen Staates*,' a paper on Vertigo, from observations made upon himself. In 1822 he was appointed to the Professorship of Physiology in the University of Breslaw, on the recommendation of Dr. Rust, of Berlin. At Easter in 1823 he entered upon his duties at Breslaw. In consequence of prejudice against Austrians on the part of the Medical Faculty, and a desire for the appointment of another person, he was not well received at first, but in the course of two years he overcame all dislike by the extent of his acquirements, and his urbane and unassuming demeanour. The inaugural disser-

tation required of the newly-appointed Professor had for its title, "*Dé examine physiologico organi visus et systematis cutanei.*" In this dissertation he described the now well-known method of investigating the structure of the retina by the appearance seen after waving a flame beside the eye. He followed out this subject in a work entitled "*Observations and Experiments on the Physiology of the Senses, or new Contributions to the Knowledge of Subjective Vision,*" published in 1825. In the course of the same year the Faculty of Medicine of Breslaw resolved to send their congratulations to Blumenbach on the fifteenth anniversary of his doctorate, accompanied by an original memoir on a suitable subject. Purkinje's offer to write the memoir was gladly accepted. He took for his subject the elementary origin of the bird's egg within the ovary, and its subsequent progress up to the time of its deposition. This investigation, which occupied him for three months, resulted in one of the most important physiological discoveries of his time, that of the germinal vesicle, now generally known as the "*vesicle of Purkinje.*" The congratulatory memoir in which it was first described and figured was afterwards published independently under the title of "*Symbolæ ad ovi avium historiam ante incubationem,*" 1830. In 1828-30 he entered upon a microscopical examination of the organization of plants. The elastic fibres of various vessels of plants, and the receptacles of pollen and seed capsules, which scatter the pollen and the seed, especially attracted his attention. The account of these researches was published in 1830: "*De cellulis antherum fibrosis nec non de granorum pollinarium formis, commentatio phytotomica.*"

On the recommendation of Mirbel, the Monthyon Prize was awarded to him by the French Institute for this essay. In the spring of 1833 he undertook the observation of the developement of the tadpole through its various stages. He carefully examined the cilia which at first cover the whole body, then the head, and lastly only the branches of the gills. In the course of the same year Professor Valentin began his researches on the organization of the ovum of mammals, and while examining the funnel of the oviduct of a rabbit, observed a movement of a granule on the mucous membrane of the oviduct while floating in water, and attributed it to spermatozoa. But Purkinje traced the motion to cilia on the edge of the membrane. This discovery of ciliary motion in a warm-blooded animal led to a joint research, and to the publication of the work '*De phenomeno generali et fundamentali motus vibratorii continui in membranis cum externis tum internis animalium plurimorum et superiorum et inferiorum ordinum obvii, commentatio physiologica,*' 1835.

On entering upon his duties at Breslaw he established a physiological institute in his own house, where students were furnished with the means of examining microscopically the elementary parts of human bodies and those of animals, and of drawing and accurately describing them. These researches supplied the materials of a series of academical dissertations, many of which disclosed new methods of microscopical investigation, a field of research then beginning to be generally cultivated after having been

comparatively neglected since the days of Malpighi, Swammerdam, and Leeuwenhoek. Others again treated of various physiological or anatomical subjects. In the first of these essays, by Krauss, in 1824, "*De cerebri læsi ad motum voluntarium relatione, certaque vertiginis directione ex certis cerebri regionibus læsis pendente*," by the introduction of his theory of vertigo, he confirmed and extended the discoveries of Flourens and Magendie respecting the activity of the cerebrum and cerebellum. In another, by Wendt, in 1833, he announced the important discovery of the sudorific glands and their excretory ducts in the human skin. In another work of this description, '*De penitiori ossium structura observationes*' (1834), by Deutsch, also in '*De penitiori dentium humanorum structura*' (1835), and in '*Meletemata circa mammalium dentium evolutionem*' (1835), by Raschkow, new discoveries made by Purkinje are announced. These were followed by '*De penitiori cartilaginum structura symbolæ*' (1836), by Meckauer, '*De arteriarum et venarum structura*' (1836), by Räuschel, '*De genitalium evolutione in embryone feminino observata*' (1837), and '*De musculari cordis structura*' (1839). In the essay entitled "*De formatione granulosa in nervis, aliisque partibus organismi animalis*" (1839), Purkinje, in opposition to the views of Remak and others, maintained that the "formatio granulosa" or elongated corpuscles resembling cell-nuclei, which are found attached to the sympathetic nerves, are not peculiar to this system, and that they consist of a newer material resembling protoplasm, out of which the more matured portions derive their growth. In the essay '*De velamentis medullæ spinalis*,' he made known the discovery of a peculiar nervous plexus distributed on the pia mater of the spinal cord, which can be easily exhibited by maceration in acetic acid. An account of these observations and of the existence of nerves in other membranous parts was given in a memoir (in Polish) published in the Year-book of the Medical Faculty of Cracow, and reproduced in German, with additions, in Müller's '*Archiv*' for 1845. Two other dissertations appeared, the titles of which were, '*De structura uteri non gravidi*' (1840), and '*De numero atque mensura microscopica fibrarum-elementarium systematis cerebrospinalis symbolæ*' (1845).

Purkinje having at length succeeded in convincing the Government of the necessity of establishing an independent institution for teaching physiology, a house was built in 1842 for the purpose of carrying on physiological researches, and a suitable grant made for defraying the stipends of assistants and other expenses. This example has since been followed in all the German and Austrian universities.

Purkinje was the author of a paper "On the World of Dreams," in '*Hesperus*' (1821), "On the Physiological Import of Vertigo" in Rust's *Magazin* (1827), "On Tartini's Tones," in the Bulletin of the Natural History Section of the '*Schlesisch-patriotischen Gesellschaft*' for 1825, and 'An Auscultation Experiment,' in which, by means of an instrument of his invention, the points of rest and motion of a vibrating plate can be de-

terminated by hearing alone, without the employment of sand, as in Chladni's experiment.

In Müller's 'Archiv für Anatomie und Physiologie' for 1834 he described a compressorium for microscopic observation invented by himself, which soon came into general use, and soon afterwards published, conjointly with Valentin, an account of researches on ciliary movements observed in the cavities of the brain. In 1838 followed his interesting experiments, in conjunction with Pappenheim, on artificial digestion. In 1845, as already stated, the same journal published his observations on the nerves. Purkinje is also the author of many valuable articles in the 'Encyclopädisches Wörterbuch der Medicinischen Wissenschaften,' and Wagner's 'Handwörterbuch der Physiologie;' of about sixty papers and lectures in the publications of the 'Schlesisch-patriotischen Gesellschaft' of Breslaw, of physiological papers (in Polish) in the scientific journals published in Cracow, of reviews and articles on the Slavonic languages and literature. He translated Tasso's 'Gerusalemme Liberata,' many of Schiller's poems, and all his Lyrics into Bohemian.

At the Naturforscherversammlung, held at Prague in 1837, he anticipated Schwann in the announcement of the doctrine of the identity of fundamental structure of plants and animals, but with this distinction between the two cases, that he calls the elements of plants and those of animals, cells and granules respectively.

In 1848 he attended the Meeting of the Slavonic races in Prague, and was present at the celebration of the five hundredth anniversary of the foundation of the University, when the Degree of Doctor of Philosophy was conferred on him. A long-cherished wish to be enabled to pass the remainder of his days in his native country was gratified by his nomination to the Professorship of Physiology in the University of Prague in the summer of 1850. His first care was the due equipment of the Physiological Institute, at that time recently established. This he effected in a satisfactory manner in the course of a year. His next endeavour was to promote the cultivation of the Natural Sciences among the Bohemian-speaking population, and with this view he became one of the editors of the Natural-History Journal 'Ziva' from 1853 to 1864, and also contributed many articles to the Journal of the Bohemian Museum.

One of the most important of his later researches was a careful investigation of the sound perceived in the interior of the skull. On examining the inmates of a deaf and dumb asylum, he found, as some previous observers had discovered, that almost all possess the power of hearing through the skull.

His election as a Foreign Member of the Royal Society took place in 1850. He was corresponding Member of the French Institute, Member of the Academies of Vienna, Berlin, and St. Petersburg, and of many other learned Societies. He retained his vigour of body and mind up to the last days of his life. His death, after an illness of no long duration, on the 28th of July, 1869, was mourned by every class of Society in Bohemia.

SIR JAMES CLARK was born at Cullen in Banffshire in December 1788, and was educated at the parish school of Fordyce, and subsequently at the University of Aberdeen. In 1806 he entered a writer's (solicitor's) office at Banff; but, not liking the law, he was given the choice of the Church, with the promise of a ministry, or the profession of medicine. He chose the latter calling, and proceeded to Edinburgh. In 1809 he passed at the College of Surgeons, and then entered the medical service of the navy. He served at Haslar Hospital till July 1810, when he was sent to sea as Assistant-Surgeon in the schooner 'Thistle,' which was going with despatches to New York. The 'Thistle' was wrecked, with the loss of several of her crew, on the coast of New Jersey, and the survivors lost everything they possessed, and suffered great privations. On returning to England he was promoted to the rank of surgeon, and joined the 'Collobrée.' It is remarkable that this vessel was also wrecked on the American coast. He was then appointed to the 'Chesapeake,' which had been recently taken by Sir Philip Broke, in his famous action, and served in her until 1814, when he was transferred to the 'Maidstone.' In this ship he met with and formed a strong friendship for Lieutenant (afterwards Sir Edward) Parry, the celebrated Arctic navigator, and made, in conjunction with him, a series of experiments on the temperature of the Gulf-stream. During his service in the navy his attention appears to have been strongly directed to the question of climate, and the few notes he has left of this period of his life chiefly refer to observations he made on this subject, and to the hygienic conditions influencing the health of the men under his charge.

In 1815 the 'Maidstone' returned to England to be paid off, and Sir James Clark was placed on half pay. In 1816 he went to Edinburgh, where he attended the University Classes, and graduated as M.D. in 1817.

In 1818 he was asked to accompany a gentleman far advanced in consumption to the south of France. He went with his patient to Marseilles, Hyères, Nice, and Florence, during the winter and spring, and in the summer to Lausanne. It was owing to this charge that his attention was especially drawn to the effect of climate on consumption, and that he commenced the collection of meteorological and climatic data, with a view of studying their influence on that disease.

In 1819 he settled in Rome, where English families were beginning to congregate, and remained there until 1826, when he removed to London.

During his residence at Rome he spent the summers in visiting the medical schools and the watering-places of Italy, France, and Germany, and continued his studies on climate. In 1820 he published a small work, entitled "Notes on Climate, Diseases, Hospitals, and Medical Schools in France, Italy, and Switzerland," which formed the foundation of a subsequent larger work on the 'Sanative Influence of Climate.' In the same year he was married to Miss Stephen, the daughter of the Rev. Dr. Stephen, Rector of Nassau, and Chaplain to the Forces at New Providence. In

1826, being partly urged to the step by his friends, and partly influenced by consideration for his wife's health, he left Rome; and after a few months spent in visiting the chief medical institutions of France and Germany, and the Pyrenean and German baths, then very little known in England, he settled in London. In the autumn of 1827 he was attacked with typhoid fever, and was ill for several months. He never recovered perfectly from this attack; it left a delicacy of digestion behind it, and permanently enfeebled him.

Soon after settling in London, Prince Leopold, afterwards King of the Belgians, whose attention had been called to him by his investigation of the German waters, appointed him his physician, and this subsequently (in 1834) led to his appointment as physician to the Duchess of Kent.

In 1829 he published his larger work on the 'Sanative Influence of Climate.' This work, which was long considered the standard book on climate, and went through several editions, has had a very wide influence, not only on medical practice, but on the collection of meteorological and other data respecting climatic conditions. He subsequently (1832) published articles on air and climate in the 'Cyclopædia of Practical Medicine.'

In the autumn of 1829 Prince Leopold, who was then engaged in the negotiation which resulted in his refusal of the crown of Greece, offered, if he accepted the crown, to take Dr. Clark to Athens; but this he declined.

He was elected a Fellow of the Royal Society in 1832, and in 1835 published his 'Treatise on Consumption and Scrofula,' which, as well as the work on climate, was translated into Italian, German, and French, and passed in this country through several editions.

Soon afterwards he wrote an article on tubercular phthisis in the 'Cyclopædia of Practical Medicine.'

Two years subsequently, on the accession of Her Majesty, he was appointed Physician in Ordinary, and subsequently received a similar appointment to Prince Albert.

From this time the life of Sir James Clark (he was made a baronet in 1838) was spent in the discharge of his responsible duties as medical adviser to the Court, and in the fatigues of a London practice. It was therefore impossible for him to continue his scientific observations on climate, or even to prosecute further his more purely professional inquiries. But indirectly, in this latter period of his life, he lent a most powerful aid to science.

He was always ready to help, and to use his influence, which yearly became greater, both with the Court and with the leaders of parties, for the furtherance of scientific objects, and for the advance of education. It is difficult to give a complete account of what he did in this direction, as he has left no records. He was, indeed, singularly indifferent to the recognition of his services, and, provided the end was gained, did not desire that his share in it should be known. But his chief influence appears to have been directed to the improvement of medical and of general education,

to fostering special scientific instruction, to the promotion of sanitary measures, to the improvement of the Lunacy Laws, and of the public medical services.

He appears to have always taken a deep interest in medical education. Early in life he had published in Italian a work addressed to Professor Tommasini on English medical literature, and some time afterwards he published some 'Observations on the System of Teaching Clinical Medicine in the University of Edinburgh, with suggestions for its improvement.' He had also corresponded with both French and Italian physicians on this point; and in the summer of 1825 he had spent several months in Paris for the purpose of observing the method of clinical teaching followed by Laennec.

When, therefore, in 1838 the University of London was founded, and he was asked to serve on the Senate, he was fully prepared to deal with this subject of medical education; and it is to a considerable extent to his labours at that time, and subsequently, when further changes were made in the curriculum, that the present examining system of the Medical Section of the University owes its shape. The leading features of the scheme which, in consultation with experienced medical teachers, he adopted, and which he advocated in the Senate, were to require evidence of a certain time having been spent in the study of medicine, but not to demand or to rely on many certificates of attendance, but to trust to a searching examination; to split up the examination into two (and subsequently into three) parts, to be undergone at different stages of education, and to make the examination as practical and as thorough as possible. Clinical examinations were not, however, at first employed, but he subsequently obtained the introduction of this important part of medical examination.

He continued to serve on the Senate until 1865, when he resigned, to the great regret of his colleagues.

In 1854, when the Government determined to open the Indian medical service to unrestricted competition, he was requested to organize the method of medical examination. He did so, and gave this examination the form which, with a slight alteration, it has since retained. In this examination he recommended the introduction of practical surgical and medical tests; and to this may be traced much of the improvement which has taken place of late years in all parts of the kingdom in practical medical teaching.

In 1858 he was appointed by the Crown a Member of the General Council of Medical Education which was constituted under the Medical Act of that year. He served on this body till December 1860.

In connexion with medical education, he interested himself on the subject of Medical Reform, and in 1842 and 1843 he wrote two letters to Sir James Graham on that subject. The second letter, which gives a *résumé* of the first, urges the need "for a good and uniform system of medical education," which he says should be the same throughout the empire for every medical practitioner. He then sketches the constitution of a body

to whom ought to be delegated the power of carrying out the principles of education to be laid down by the Government. He had evidently formed an idea of a General Medical Council, which may yet some day be turned to account.

He did not, however, restrict his labours to medical education. He took a deep interest in the improvement of the Universities generally, and assisted Prince Albert in the projects which eventually ended in the alterations in the Universities of Cambridge and Oxford. At a later date he was very active in aiding the reconstruction of the University of Aberdeen.

His greatest attempt to improve purely scientific education was made in connexion with the College of Chemistry. He was deeply impressed with the defective opportunities of studying practical chemistry in this country as compared with Germany, and with the unfavourable influence that deficiency would have, not only on our scientific standing, but on our powers as a manufacturing nation. The influence of Liebig's doctrines on agricultural chemistry and on the improvement of the productive powers of soil were also at that time attracting great attention in England, and impressed him greatly with the importance of cultivating this subject. Whether the State should or should not more or less assist the teaching of pure science, or should leave this to the independent exertion of institutions or private individuals, is a matter which need not be here discussed. Sir James Clark's opinion appears to have been that the Continental system of State aid had the effect of overweighting England in the race, and that if we wished to maintain our equality in science, we had no option but to imitate to a certain extent the Continental plan. The College of Chemistry, however, in the first instance, was intended to be self-supporting. It was commenced in 1845 by Dr. Gardner; and Sir James Clark soon became one of its most active supporters, and through his influence Prince Albert interested himself greatly in it.

In the summer of that year, when the Queen and Prince were in Germany, Professor von Liebig was requested by Sir James to name some chemist who could carry on in England the same kind of practical instruction which had made Giessen so famous. Liebig mentioned three names, and fortunately circumstances led to the selection of Dr. Hofmann. Through the influence of Prince Albert, Dr. Hofmann obtained leave from the University of Bonn for two years, and soon afterwards the College of Chemistry was opened.

How successful it was in a scientific point of view, even from the first, needs no record; but its expenses were heavy, and perhaps the College might even have been closed from pecuniary failure about the year 1852 had not the Prince Consort, urged on by Sir James, so exerted his influence that the Government consented to give a small assistance, and at length the College of Chemistry eventually became incorporated with the Royal School of Mines. Since that time the College (which is partly self-supporting) has done much to diffuse among our manufacturing and agri-

cultural population a knowledge of Chemistry, and to advance the science by original research. It is to be regretted, however, that the College of Chemistry, originally established as an independent institution, self-supporting, or aided, if necessary, by private means, could not maintain itself on that footing.

As far as possible also Sir James Clark gave a warm support to all plans for promoting the study of Natural History, and was ready to urge on the Government at any time any reasonable mode of doing this, or of furthering independent inquiries.

Passing from pure science, he had a great share in the sanitary movement which has been so marked a feature of our days, although his name was not brought before the public so prominently as that of others who had really less influence. From a very early period he had been a very strong advocate of measures calculated to prevent disease and to improve the public health. He therefore used his influence with the Government to institute the Health of Towns' Commission, and those other early inquiries which were the foundation of the present movement. He was at this time intimately acquainted both with Andrew and George Combe, and estimated very highly the philosophical characters of the two brothers. Some years afterwards he edited and partly rewrote one of Andrew Combe's Hygienic works on the Management of Infancy.

At a very early date also, long before the Crimean war, he did what he could to get the sanitary state of the army and navy inquired into and remedied.

There can be no doubt that his service in the Navy had impressed him with the urgent importance of this subject, and had also given him a strong conviction of the waste of life in warlike operations.

Owing probably to their knowledge of his exertions in this direction, the Government during the Crimean war requested his cooperation in the organization of Supplementary Civil Hospitals, in support of the Military Hospitals, which were overflowing and had proved unequal to the work entailed by a severe campaign. He assisted in the deliberations which resulted in the establishment of the Smyrna Hospital; and subsequently, when a second hospital was required, the Government requested him to undertake the entire organization. He did so, and the result was the great Hospital of Renkioi on the Dardanelles, which was intended for 3000 sick. This hospital, the design of which was made by Mr. Brunel, has proved the model of the American Wooden Hospitals established during the late civil war, and indirectly has given rise to many of the arrangements in field hospitals in war which were carried out in Italy and Germany in the campaigns of 1859 and 1866, and are now being repeated on a still larger scale.

It was therefore not surprising that after the Crimean war he was asked to serve on the Royal Commission, presided over by Mr. Sidney Herbert, for inquiring into the health of the army; and he had no small share in

shaping the conclusions arrived at in that well-known and important inquiry. He subsequently took an equal interest in the Indian Sanitary Commission; and it is really chiefly to his exertions and his influence with the Government (in support of the persistent action of Miss Nightingale, Sir Ranald Martin, Dr. Sutherland, and others) that we must attribute the advance which has been made in carrying out that most important reform, a reform which will influence not only the European soldiers in India, but the many million inhabitants of that empire.

It is not wished to claim for Sir James Clark more honour than is due. There were many other labourers in the field, and no one man unassisted could have done such great works. All that is urged for him is that he was one of the earliest of those who saw the importance of sanitary science, and that he was ever ready with time and thought and influence to aid in the progress of inquiry and reform. In connexion with military medical arrangements, he served on the Committee which organized the Army Medical School now stationed at Netley; and he continued to the last moment to take the warmest interest in everything connected with that institution.

In addition to the work of inquiry on sanitary legislation among the civil population and in the public services, he was very much interested in the legislation for the insane. In 1855 an American lady, Miss Dix, who was visiting the lunatic asylums of England and Scotland, was refused admission into some of the private asylums in the latter country. In order to compass her wishes, she obtained introductions to some influential persons, among others to Sir James Clark, and the inquiries then set on foot led to the appointment of a Royal Commission to inquire into the Scotch Lunacy Laws. In this inquiry, and in the appointment of the Lunacy Commissioners which followed the Report of the Royal Commission, Sir James Clark took an active share; and in after years, when various attempts were made to revert to the old state of things, he spared neither time nor trouble to stem the retrograde current by correspondence and verbal remonstrance with Members of Parliament and Members of the Cabinet; indeed, after his death, the Lord-Advocate quoted in Parliament a letter from him as a justification of the foundation of the Scotch Lunacy Board.

Only two years before his death he wrote a life of Dr. Conolly, the object of which was not only to perpetuate the memory of his friend, but also to place before the public the true treatment of the insane, and to rebut the attempts, certainly feeble enough, which have been made to impair the wise and benevolent mode of treatment which Conolly did so much to popularize.

When it is considered that all these labours (and in the true sense of the word his exertions were labours) were carried on in addition to the work entailed by his Court duties and a large private practice, the great activity of Sir James Clark will be appreciated.

In this sketch only some of the public services rendered by him can be

referred to ; for his mode of using his influence was so unostentatious, and his desire for a recognition of his services so small, that much of what he did is scarcely known ; and the want of specific details in showing how his influence was brought to bear in so many ways is owing to the modesty of his nature. Justice, too, has hardly been done in the foregoing lines to his scientific knowledge and sympathies. In this respect, as in his constant endeavour to promote the wellbeing of his fellowmen, he was so little self-obtrusive that few men knew the extent of his acquirements. He paid, even to within a week of his death, constant attention to scientific progress, and especially to its practical application. Among his notes written but a few weeks before his last illness are details of the composition and mode of action of chloral. It was this union of a scientific spirit with great benevolence of character which, aided by a large experience abroad and at home, made him so excellent a physician.

His position at the Court necessarily occupied much of his time and thoughts ; he was unceasing in his attention to the health of the Queen and of her children, and the Royal family owe to him much of that blessing of health which has happily been their lot. He was on most confidential terms with the Prince Consort ; and the Prince found in him a congenial adviser on all points connected with education and science. The Queen's trust in him was early and firmly implanted, and was never impaired, and her sympathy and, we can truly say, affection for him were manifested to the last.

Sir James Clark retired from private practice in 1860, and removed to Bagshot Park, which Her Majesty had lent him for his life. He died there on the 29th of June, 1870, in the eighty-second year of his age, retaining almost to the last hour of his life a warm interest in all scientific progress, and a heart-felt sympathy with every step which would promote the improvement and happiness of his fellowmen.

WILLIAM ALLEN MILLER, Vice-President and Treasurer of the Royal Society, was born at Ipswich, in Suffolk, on the 17th of December, 1817. He was indebted for his early education to his mother, whose memory he cherished with the greatest love and respect, and whose quiet, sagacious nature was reflected in him. Mrs. Miller had a favourite maxim, "Take everything by the smooth handle ; and if a thing has not got a smooth handle, make one !" Dr. Miller was actuated through life by the spirit of this axiom ; and we have known him, when giving advice to a friend who sought it, introduce the remark, "Take it by the smooth handle."

Miller passed one year in Merchant Taylors' School, and two years at Ackworth, in Yorkshire, in a school belonging to the Society of Friends—the same in which Luke Howard took so great an interest that he purchased the Ackworth Villa estate, and made it his summer residence during some years. Luke Howard's partner, William Allen, F.R.S., the manufacturing chemist, and author, conjointly with Mr. Pepys, of the well-known researches

on respiration, was the friend after whom Miller was named. Miller's natural simplicity of character probably received its outward expression from this early contact with influential members of the Society of Friends. It was at Ackworth that he first distinctly remembered having acquired a taste for science, and a desire to devote his life to its cultivation; and this was not so much from the chemical lectures, or rather the chemical experiments, which were shown to some of the boys, as from the fact that Miller was occasionally invited to look at the stars through a telescope belonging to one of the masters. These early impressions bore fruit in the chemistry of the stars, with which his name is now associated. At the age of 15 he was apprenticed to his uncle, Mr. Bowyer Vaux, one of the honorary surgeons in the General Hospital at Birmingham, of which, during nearly twenty years, his father, Mr. William Miller, was secretary. After five years he entered the medical department of King's College, London, where his superior knowledge of chemistry over that of the other students attracted the attention of Professor Daniell, who more than once expressed his surprise in the inquiry, "Where did you get your knowledge from?" One of those opportunities that occur in the lives of most people, but are taken advantage of only by superior men, occurred in connexion with the chemistry lectures. Miller had no taste for surgical practice, and preferred, if possible, to get some employment in the laboratory of a manufacturing chemist, rather than become a medical practitioner. Indeed he did perform some analyses for the Messrs. Chance, while in treaty with them for more permanent employment. But the laboratory assistant at King's College having been disabled by illness, Daniell engaged the services of Miller; and when the office of Demonstrator in the laboratory became vacant in 1840, he was appointed to the post. It should be mentioned that in 1839 Miller obtained the Warneford Prize for the encouragement of theological studies among medical students, and in 1840 he passed a few months in Liebig's laboratory at Giessen. In 1841 he became Assistant Lecturer for Professor Daniell, and also took his degree of M.B. in the University of London, proceeding to M.D. the following year. He also assisted Professor Daniell in various scientific inquiries, and conducted the experiments on the electrolysis of saline compounds, his name being associated with that of Daniell in the paper that appeared in the *Philosophical Transactions* for 1844. In the following year he was elected a Fellow of the Royal Society, and on the death of Professor Daniell succeeded to the vacant chair of Chemistry in King's College. The writer of this notice was engaged in assisting Professor Daniell to bring out the third edition of his well-known work entitled "*Meteorological Essays*," and on the sudden death of the author he requested Dr. Miller to cooperate with him in completing the work, to which he readily assented. Dr. Miller was engaged about this time in some experiments on *Spectrum Analysis*. They were conducted in a sort of lumber-room below the seats of the Chemical Theatre, and formed the subject of a paper which was

read before the British Association and published in 1845 in the *Philosophical Magazine* (Series 3, vol. xxvii. p. 81). He thus became interested at an early period in the subject of spectrum investigation.

Miller continued during some years to use as his text-book Professor Daniell's 'Introduction to the study of Chemical Philosophy,' supplemented at a later period by Fownes's 'Manual of Chemistry.' The writer of this notice repeatedly urged Dr. Miller to bring out a work of his own, which should be better suited to the wants of his pupils; but he hesitated in doing so lest he should at all interfere with his predecessor's work. "I must prepare the book," he said, "from my lecture-notes, and you are not aware how much of Daniell I have in them." For some time his idea was to accede to a proposal of the publisher of Daniell's work to bring out a third edition, making such additions thereto as the progress of science required, and to maintain it in its old position as the text-book. But on looking over Daniell with this view, he found that so many additions and alterations would be required as greatly to supersede the author's peculiar touches; so that he finally decided to produce a new work, and the first volume accordingly appeared in 1855. In the preface to this volume, which was devoted to "Chemical Physics," the author stated that "he had decided to leave untouched the work of his late master, as the true exponent of his views, upon some of those branches of science which his researches had contributed to advance and adorn." The two subsequent volumes on "Inorganic" and "Organic Chemistry," which appeared, the one in 1856 and the other in 1857, were written from Miller's lecture-notes, as was also the case with the "Chemical Physics," the notes being so amplified as to form continuous reading, a process which led to so many insertions and alterations as to make the manuscript difficult to read. But the effect of this mode of treatment was so far advantageous that when the books were introduced to the students, they, so far from having to conform to any new method, seemed to recognize in the new text-books the very lectures they had heard.

The three volumes of Miller's 'Elements of Chemistry' passed through several editions, and were reprinted in the United States of America. While not professing to set forth any marked original views, the work affords a clear and comprehensive exposition of the science, and soon became deservedly popular. In the later editions Miller adopted the new method of notation in chemistry. His conservative principles led him to resist this change as long as it was possible to do so. Moreover, having been, during so many years, accustomed to the old notation, he never took kindly to the new. Indeed it was part of Miller's character to grasp a new idea with a certain amount of mental slowness; but when once fairly appreciated, it was held tenaciously and not given up without a severe struggle. But he was so conscientious that he would sacrifice every thing to what he held to be the truth. The writer has known him to refuse to hold any further intercourse with a foreign man of science whom he had received into his

house and assisted in various ways, on hearing the expression of a doubt as to the benevolence of the Almighty for permitting him to undergo so much trouble. Soon after he became Professor, he was on one occasion giving evidence in a court of law on some scientific point connected with a patent, when, during the cross examination, the Judge made a remark which had the effect of questioning the veracity of the witness. Miller felt this so keenly that he fainted, and had to be carried out of court. After a short interval the Judge sent to inquire how he was. Miller said, "I shall be better when his Lordship does me justice." On his return, the counsel for the cross examination was proceeding to put questions in the spirit of the objection, when the Judge stopped him, stating that he had misunderstood the witness, and explained how.

As a lecturer Miller was more successful in style and expression than as a writer, for his written composition had some tendency to become involved. One of the best specimens of his lectures is that on Spectrum Analysis, given before the British Association at Manchester in 1861, at the time when Kirchhoff's researches had made the subject more than usually popular. One part of this lecture was devoted to an historical review of that remarkable branch of chemico-physical research; and so little attention had been paid to this part of the subject that when a large audience were collected to hear, as they supposed, an account of Kirchhoff's discoveries, they were not a little surprised to find Kirchhoff occupying the end of a long series of illustrious names, from Newton in 1701 to Wollaston in 1802 and Fraunhofer in 1815; while the various other names were arranged after the fashion of a genealogical tree, under the four heads of (1) Cosmical lines, (2) Absorption-bands, (3) Bright lines produced by the electric spark, and (4) by coloured flames, the four branches uniting in the names of Kirchhoff and Bunsen, 1860. On the morning of the day appointed for that lecture, successful and brilliant as it was, Dr. Miller was seized with one of those bilious attacks to which he was subject, and was so prostrated that he had to keep his bed nearly up to the time of the lecture, and return to it immediately after its close. This gave occasion to a little incident which deserves to be noted as illustrative of the cautious habit of forethought of the man. In moving to the front of the crowded platform with a bottle containing red nitrous fumes in his hand, in his weak state he stumbled and fell, breaking the bottle in pieces. Immediately he sprang to his feet, exclaiming "I have another!" on which a round of applause caused him to remark, as if to himself, "I am too old a lecturer to rely upon one bottle."

This lecture was repeated before the Pharmaceutical Society of London on the evening of the 15th January, 1862, and printed in the Society's Journal for February of that year. The historical details given in it have been largely used by subsequent writers, presenting, as they do, in a very clear manner, the results obtained by the earlier workers on the Spectrum. It was on returning from this lecture to his house at Tulse Hill, with his

friend and neighbour Mr. (now Dr.) Huggins, that Miller assented to a proposal made by Mr. Huggins that they should unite in carrying on a series of experiments on the spectra of the heavenly bodies. Miller was at this time engaged in an elaborate series of experiments which formed the subject of a paper read before the Royal Society, 19th June, 1862, "On the Photographic Transparency of various bodies, and on the Photographic effects of metallic and other Spectra obtained by means of the Electric Spark." This paper is inserted in the Philosophical Transactions for 1862.

The joint labours of Miller and Huggins were continued during about two years; and as the observations could only be made at night, they must have told on the energies of a man who was so actively employed as Miller in brain-work at College and elsewhere during the day. The first results of their observations are given in a note on the lines in the spectra of some of the fixed stars, dated February 1863*, from which it appears that a considerable time was devoted to the construction of apparatus suited to this delicate branch of inquiry; but they had at length "succeeded in contriving an arrangement which has enabled them to view the lines in the stellar spectra in much greater detail than has been figured or described by any previous observer." They further add that, "during the past twelve months, they have examined the spectra of the Moon, Jupiter, and Mars, as well as of between thirty and forty stars, including those of Arcturus, Castor, α Lyræ, Capella, and Procyon, some of the principal lines of which they have measured approximatively. They have also observed β and γ Andromedæ, α , β , ϵ , and η Pegasi, Rigel, η Orionis, β Aurigæ, Pollux, γ Geminorum, α , γ , and ϵ Cygni, α Trianguli, ϵ , ζ , and η Ursæ Majoris, α , β , γ , ϵ , and η Cassiopeïæ, and some others." When their labours were sufficiently advanced, they embodied their results in a memoir entitled "On the Spectra of some of the Fixed Stars," which was published in the Philosophical Transactions for 1864. At a somewhat later period there is a joint note on the spectrum of the variable star Alpha Orionis, contained in the 'Monthly Notices of the Royal Astronomical Society,' vol. xxvi.; and another joint note on the spectrum of a new star in Corona borealis, in the 'Proceedings of the Royal Society,' vol. xv., dated May 17, 1866.

Messrs. Miller and Huggins, for their "conjoint discoveries in Astronomical Physics," received each the Gold Medal of the Royal Astronomical Society; and on the occasion of presenting these medals, the President, the Rev. C. Pritchard, M.A., F.R.S., after referring to a former inquiry as to "What is a sun?" remarked that the progress of science had led to the further query, "What is a star?" "For the first dawning of a distinct and intelligent reply to this question we are indebted to Messrs. Huggins and Miller." * * * We find them associated "in the examination of the spectra of stars by means of an admirable and newly contrived apparatus which had required much thought and labour to construct. With this

* Proceedings of the Royal Society, vol. xii. p. 444.

instrument attached to the telescope it was possible not only readily to divide the sodium line D into its two compartments, but to exhibit also the nickel line which Kirchhoff had observed between them. The spectra of the stars were now, in the first instance, compared approximately with the superposed atmospheric spectrum already alluded to, for the purpose of suggesting what metallic lines probably existed in the star under observation, and then were compared directly, by actual juxtaposition, with the actual spectra of those metallic vapours which had been already suggested. It seems impossible to conceive any process more rigidly or conscientiously exact than that which Messrs. Huggins and Miller thus skilfully adopted; and here I may be excused for repeating that the attainment of the ultimate object of the research depended, not on any approximation, however close, of the stellar with the metallic spectra, but on the certainty of their absolute coincidence. In this way, during the space of two years and a quarter, many of the midnight hours of these gentlemen were passed in the scrupulous examination and measurement of the spectra of upwards of fifty stars; but in several instances the number of the fine dark lines, the inevitable indices (be it remembered) of the material constitution of these distant worlds, were so numerous, that to measure and map them all the labour of months would barely suffice. The physical result of all this scrupulous and conscientious care was to discover the fact, or it may be to confirm the suspicion, that those mysterious lights with which the firmament is spangled are in strict reality worlds fashioned, in their material constitution at least, not altogether differently from the fashion of the little orb on which we live; beyond the question of a doubt they are proved, by the investigations of our medallists, to contain at least the hydrogen, the sodium, the magnesium, the iron with which all terrestrial creatures are so familiar."

On Tuesday, May 14th, 1867, Miller commenced at the Royal Institution a course of four lectures on "Spectrum Analysis, with its applications to Astronomy." These lectures were reported in the 'Chemical News,' under the revision of the author. Again, at the Meeting of the British Association at Exeter in 1869, he gave a lecture on Spectrum Analysis to working men. This lecture was afterwards published in the 'Popular Science Review' for October 1869.

Miller was interested in the subject of water analysis, and, in conjunction with Professors Graham and Hofmann, prepared a Report for the Government "On the Chemical quality of the Supply of Water to the Metropolis." This was printed in 1851. At a later period he undertook an investigation "On the combined Action of Air and Water on Lead," and in 1865 gave a lecture before the Chemical Society "On some points in the Analysis of Potable Waters."

Quitting the subject of Miller's original work, we pass on to a brief notice of the various services rendered by him to Science. He was on the Council of the Royal Society during the years 1848-50 and 1855-57,

and being elected Treasurer on the 30th of November, 1861, he served on the Council in an official capacity till the time of his death. His methodical and punctual habits, his knowledge of affairs, and his excellent judgment, with the earnest and lively interest he took in the welfare of the Society, rendered his special services as Treasurer of the utmost value; whilst the same high qualities, combined with his accomplishment in science, singularly well fitted him for the various duties he had to perform as Member of the Council and a chief Officer of the Society. The date of his election to the Fellowship is 1845.

Mr. Gassiot, who knew Miller intimately, referring to his merits as Treasurer, writes, "a more straightforward officer, or one more devoted to upholding the dignity and promoting the usefulness of the Royal Society, I do not know; and there is probably no one with whom Dr. Miller communicated on the subject so freely as myself." Mr. White, the Assistant-Secretary, who habitually had to transact business with him, adds, "My own experience of Dr. Miller was, that on walking down to King's College I could tell beforehand the mood in which I should find him—always uniform and considerate. His decision on questions brought before him was generally quick and sound, and he was ready in detecting the weak points of an argument. In the whole period that he was Treasurer I never had a single disagreeable word with him."

In 1866 Dr. Miller was nominated a Member of the Committee then appointed for the purpose of superintending the Meteorological Observations made by direction of the Board of Trade, and served on it till the time of his death; he was also an active Member of the Committee of the British Association for superintending the Kew Observatory, and devoted much time to that work. The definition of the arrears to be executed under the superintendence of Mr. Balfour Stewart, as entered on the Minutes of the Committee, 9th March, 1870, was written by and inserted at Dr. Miller's particular request, in order specially to define the important work that has yet to be completed by the time when the connexion of the Observatory with the British Association shall cease.

It may here be mentioned that whilst a Member of the Committee appointed to advise on the scientific arrangements for the marine researches carried on during the voyage of the 'Porcupine' in 1869, Dr. Miller was happy enough to contrive a thermometer adapted for taking deep-sea temperatures, which has been found admirably to fulfil its purpose*.

Miller was one of the original founders of the Chemical Society, and frequently presided over its meetings, as well as occupied a place at its Council Board. Along with his other various occupations, Dr. Miller was a Member of the Senate of the University of London, to which he was appointed, on the recommendation of Convocation, early in 1865; and his sound judgment and knowledge generally, as well as his accomplishment in chemical and physical science and his experience as a teacher, gave

* Proceedings of the Royal Society, vol. xviii. p. 408.

great weight to his opinion in the deliberations of that body, and caused his loss to be severely felt.

In addition to the various honours which rewarded Miller's position as a scientific man, it should be mentioned that he received the degree of LL.D. at the University of Edinburgh, on the occasion of the installation of Lord Brougham as the first Chancellor, that of D.C.L. at the University of Oxford in June 1868, and that of LL.D. at the University of Cambridge in May 1869, after giving the Reade Lecture, which on this occasion was on the Coal-tar Colours.

Perhaps the most marked feature in Miller's character was sagacity combined with a deep sense of religion. His religious views may be gathered, although imperfectly, from an address entitled "The Bible and Science," delivered at the Church Congress in Wolverhampton, October 3rd, 1867; also from his "Introductory Lecture," on the opening of the Medical Session at King's College, October 1st, 1859, published under the title, "Hints to the Student on commencing his Medical Studies."

In conclusion, the writer may be allowed to repeat what he said in a short notice at the period of Miller's death, drawn up at the request of the editor of the 'Chemical News':—"During a quarter of a century Miller continued to lecture with unceasing activity, and to take part in the management of King's College, every one, from the Principal and Professors to the youngest student, being anxious to obtain his advice and assistance. It was impossible to come in contact with him without feeling one's self in the presence of a man of pure nature, of spotless integrity, of sound and sagacious judgment, and of true gentlemanly feeling. His loss will be deeply felt, especially in King's College, in the Royal Society, in the Mint, and the Bank of England, where he was one of the Assayers. He will be missed in the Courts of Law, where his clear perception of patented processes, and his strong sense of justice, made him respected alike by judge and counsel. He will be missed by the manufacturers who sought his advice; but, above all, he will be missed by his own family, and by the few friends who had his confidence."

There had been symptoms of an overwrought brain for some months previous to his last illness, which took place on the journey to Liverpool, 13th September, 1870, at the time of the British Association gathering, which, however, he was unable to attend, his illness culminating in apoplexy on the 30th of the same month. His remains were brought from Liverpool and interred in the cemetery at Norwood, by the side of those of his wife, whom he survived one year. He died on the anniversary of her burial, and at the comparatively early age of 53. He married, in 1842, Eliza, eldest daughter of the late Mr. Edward Forrest, of Birmingham, by whom he leaves issue, two daughters and one son.—C. T.

JOHN T. GRAVES, M.A., F.R.S., was son of John C. Graves, of Dublin, Barrister-at-Law. He was born in Dublin on the 4th of December, 1806, and passed some years in the school of the Rev. Samuel Field, Westbury-on-Trym, Somersetshire. He entered Trinity College, Dublin, in 1823, and was a class-fellow of Sir William Rowan Hamilton, with whom, though living at a distance, he kept up a life-long friendship. In his undergraduate career he was distinguished in both Science and Classics, and at his Degree Examination in 1827 was awarded the Classical Gold Medal. He soon after took an *ad eundem* degree at Oxford, and was incorporated in Oriel College, where he resided some time, and proceeded to the degree of M.A. He was also M.A. of Dublin University. On the 10th of June, 1831, he was called to the Bar as a Member of the Inner Temple, and for a short time went the Western circuit. In the year 1839 he was appointed Professor of Jurisprudence in University College, London, in succession to Mr. Austin, and not long after was elected to be Examiner in Laws in the University of London. The records of his work as a lawyer are Twelve Lectures on the Law of Nations, published in the 'Law Times,' commencing April 25, 1845, and two elaborate articles contributed to the 'Encyclopædia Metropolitana,' on Roman Law and Canon Law. About this time he was a contributor to Smith's Dictionary of Greek and Roman Biography and Mythology. Among other articles from his pen are those on Cato, Crassus, Drusus, Gaius, and the Legislation of Justinian.

As a scientific author Mr. Graves commenced his labours in his twentieth year. It was in October 1826 that he was engaged in researches on profound and subtle questions in analysis; the results he obtained were communicated to the Royal Society of London in the year 1828, and published in the Philosophical Transactions for 1829, under the title "An attempt to rectify the Inaccuracy of some Logarithmic Formulæ." This paper gave rise to interesting and important discussions, with which the names of M. Vincent, Peacock, Ohm, De Morgan, Warren, Rowan Hamilton, and others are connected. It was by meditating upon the results of this memoir that Sir W. Rowan Hamilton was led to his ingenious theory of Conjugate Functions or Algebraic Couples, as may be learned from Sir W. R. Hamilton's abstract of a paper "On Conjugate Functions, or Algebraic Couples, as tending to illustrate generally the Doctrines of Imaginary Quantities, and as confirming the Results of Mr. Graves respecting the existence of two Independent Integers in the complete expression of an Imaginary Logarithm," as well as from an abstract of a "Memoir on the Theory of Exponential Functions," both published in the Report of the British Association for 1834. In continuation of the same and allied researches, Mr. Graves contributed a paper to the Philosophical Magazine for April 1836, "On the lately Proposed Logarithms of Unity, in reply to Prof. De Morgan;" and in November and December of the same year another, entitled "Explanation of a remarkable Paradox in the Calculus of Functions, noticed by Mr. Babbage." To the same journal were contri-

buted by him, in September 1838, a New and General Solution of Cubic Equations; in August 1839 a paper on the Functional Symmetry exhibited in the Notation of certain Geometrical Porisms when they are stated merely with reference to the arrangement of points; and in April 1845 a paper on a Connexion between the General Theory of Normal Couples and the Theory of Complete Quadratic Functions of Two Variables. A subsequent number contains a contribution on the Rev. J. G. MacVicar's Experiment on Vision; and the Report of the Cheltenham Meeting of the British Association contains abstracts of papers communicated by him on the Polyhedron of Forces, and on the Congruence $nx = n + 1 \pmod{p}$.

The above list of papers, itself incomplete, is far from representing adequately Mr. J. T. Graves's contributions to mathematical science. The Transactions of the Royal Irish Academy contain many traces of his intellectual activity; and by his long correspondence with Sir William Rowan Hamilton, commenced at an early period and maintained until death interposed, Mr. Graves may be said to have taken no small part in bringing to maturity the splendid conception of Quaternions, by which alone the name of Hamilton would have been rendered immortal. In his preface to the 'Lectures on Quaternions,' Sir William makes frequent allusion to the suggestive character of his correspondence with his early friend, and warmly expresses his indebtedness thereto.

Mr. Graves was one of the Committee of the Society for the Diffusion of Useful Knowledge. In the year 1839 he was elected a Fellow of the Royal Society, and he subsequently served upon its Council. He was also a Member of the Philological Society and of the Royal Society of Literature.

For many years past he had taken interest in forming a collection of mathematical works of all ages and countries, a collection which, though only to be appreciated by the few, is by those qualified, who are acquainted with it, considered to be almost unique for historical curiousness and completeness; and nearly every book composing it was bound under his direction with costly care and elegance. This portion of his library he bequeathed to University College, London, in remembrance of his former connexion as Professor with that Institution.

In the year 1846, soon after his marriage with the daughter of the late William Tooke, Esq., F.R.S., he was appointed Assistant Poor-Law Commissioner, and on the constitution of the present Board in 1847 was made Poor-Law Inspector. He served efficiently in that department till the past month, when he sent in his resignation, an act which he did not long survive. He died on the 29th of March, at his residence in Cheltenham, at the age of 63.

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